

# RADICAL SCIENCE JOURNAL 9

1979

£1.50

The aim of this journal is to provide a forum for extended analyses of the ideology and practice of science and technology from a radical political perspective. Most contributors have attempted to re-examine past Marxist views and to develop a Marxist critique of the role of scientism in the left.

The Journal is edited and produced collectively. This issue was put together by: Graz Baran, Jane Barker, Mike Barnett, Philip Boys, Gavin Browning, David Dickson, Karl Figlio, Mike Hales, Luke Hodgkin, Brian Hurwitz, Chris Knee, Les Levidow, Maureen McNeil, David Murray, Gianna Pomata, Barry Richards, Tony Solomonides, Fred Steward, Margot Waddell, Bob Young and Michael Young.

The subscription for three consecutive issues is £4.50 post paid. Library and institutional subscriptions are £10.80. *RSJ* appears at irregular intervals; on account of this, the subscription is *not* annual but covers three consecutive issues. For details of back numbers see inside back cover.

Those who support us in our project are invited to add a donation to their subscription. Subscriptions, donations, enquiries and articles for consideration should be sent to: Radical Science Journal, 9 Poland Street, London W1V 3DG. **Please add the equivalent of 60p to foreign cheques to cover the additional bank charges involved.**

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*RSJ* is typeset by Bread 'n Roses, 30 Camden Road, London NW1 and printed by Russell Press, Bertrand Russell House, Gamble Street, Nottingham NG7 4ET.

*RSJ* is listed in the *Alternative Press Index*. Articles are abstracted in *Sociological Abstracts* (P.O. Box 22206, San Diego, CA. 92122, USA), from whom photocopies are also available.

*RSJ* is distributed by:

Britain: PDC, 27 Clerkenwell Close, London EC1, tel. 01-251 4976.

USA: Carrier Pigeon, 75 Kneeland St., Rm 309, Boston, MA 02111, USA,  
tel. 617-542 5679.

ISSN 0305 0963

**RSJ 8 Errata:**

p.91: A new paragraph should begin at the bottom of the page with, 'In writing a collective biography, Werskey has undertaken a deceptively difficult task: he has attempted to mesh together the lives of five individuals . . . (continues on p.92).'

p.103, line 12: for 'socialist', read 'sociologist'.

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# EDITORIAL

Most of RSJ 9 is given over to medicine and medically related issues. Perhaps it would be more accurate to say that, if we take one of the major forms of scientific ideology to be the reduction of quality to quantity, then the particular form of that mystification which the articles in this issue critique is the reduction embodied in the dichotomy between the normal and the pathological. How are the limits of the normal defined, beyond which a condition is taken to be pathological? What is the coercive power attached to each category? What political, economic and scientific forces maintain them, along with presumed 'medical' aetiologies (causes of illness) and 'medical' responses.

The notion of illness as a risk, which runs through several of the articles, exemplifies what we mean by the reduction of quality to quantity embedded in the normal and the pathological. Such a notion of illness rests upon the unarticulated assumption (more a perception) that illness is a kind of accident which happens to exposed individuals in a society itself taken to be merely the aggregate of separate individuals. Within such a framework, there can be, at a conceptual level, no social aetiology of illness, and, at the level of action, no collective mobilization around illness as a social phenomenon. In such a society, 'threshold limit values', 'reasonable risks' and 'safe levels' displace the experience of health and ill-health and structure the forms of resistance, as, for example, when resistance takes the form of disputing how high a risk is or should be.

The second theme which runs through this issue has to do with theory and practice. The articles address themselves quite specifically to what was done and not done (e.g. at Seveso), could be done and could not be done, might be done and might not be done. The issue begins with an open letter from Mike Hales, who criticizes the 'detached partisanship' of left intellectuals. He does not call for the abolition of theory, but rather advocates the use of theory (and hence the activity of its formulators) to open up, nourish, and integrate with, concrete practices. The letter was written in response to the Collective's criticism of his review of Philip Kraft's book, *Programmers and Managers* (also in this issue). We felt that Mike Hales' recent opportunities for political struggle which combine theory and practice had coloured his first draft of the review of a book which had a very different political context. But as a consequence of having separated the review from the critique of detached partisanship, there now arises the problem of relating intellectual analyses to political struggles.

These two themes (the coerciveness in the normal and the pathological, and the relating of theory and practice) are brought together in the main article for this issue, 'Sinister Medicine?', by Karl Figlio. He criticizes the problematic framework held in common by various leading critical approaches to medicine, which implies an (often unintended) utopianism not amenable to struggles. He tries to get beyond having to attribute the evils of medicine under capitalism to 'sinister' medicine as such, and attempts to characterize what it means to call medicine 'capitalist' in the first place.

The related theme of 'safe level' is taken up in three other articles, each exemplifying how medical categories lend themselves to representing capital's power of social manipulation as technical properties of individuals — in this case, a 'threshold limit' of contamination below which the individual supposedly suffers no damage. At Seveso, a very limited evacuation — largely determined by political and publicity considerations — was presented as a technical judgement, but lost credibility with the affected population who were able to compare for themselves the harmful effects of dioxin inside and outside the official evacuation area. In 'Seveso: Safety in Numbers?', Gianna Pomata describes the contradictions of the Italian state needing to enforce its limited evacuation so as to head off demands for a wider one.

At Three Mile Island, where the surrounding population could judge the dangers only with geiger counters, doubts over the official limited evacuation arose in a more abstract and speculative way; the authorities tried to concretize the issue by appealing to the benign metaphor of medical X-rays, reassuring the population about the low radiation dose per person. Capital's task of crisis management — in this case, containing the potential disruption — forms the theme of Les Levidow's account of 'Three Mile Island: the Ideology of "Safe Level" as a Material Force'.

Similarly, the continuing, omnipresent catastrophe of industrial pollution often gets mystified in the workplace through disputes over how low to set 'Threshold Limit Value', below which few are supposed to be suffering any damage. We had intended to publish 'The Politics of the Science of Lead Pollution', in which Mike Peters mounts a counter-attack on the TLV-based forms of scientific arguments used to parry criticisms levelled against lead from petrol. Because of lack of space, his piece will appear in *RSJ* 10

Also related to Karl Figlio's article are two replies to the letter we published in *RSJ* 8 from John Stewart, 'Scientific Findings That Look Awkward for Socialists: How Are We to Respond?'. The political quandary he had posed for socialist scientists exemplifies the power of capitalist medicine to constitute particular diseases (e.g. schizophrenia) out of

illness — thereby throwing up the mystified problem of how to demarcate the social/biological bases of those diseases. In this issue, Michael Joseph attempts to solve that political quandary within John Stewart's terms of reference, while Les Levidow suggests how we might transcend them.

Sohn-Rethel's materialist investigations into the social basis of the 'hardest' and most abstract sciences — physics and mathematics — have loomed large in the project of our journal, as in our presentation of his ideas in RSJ 2/3. We now welcome the appearance — for the first time in English — of a fuller version of his perspective, *Intellectual and Manual Labour*, reviewed in this issue by Chris Knee from the vantage point of his own ongoing project on the sociology of geometry.

In a category of writing which we hope to expand in future issues, we present two accounts of science as personally lived contradictions: Daniel Schiff on the teaching of 'Hard Spheres', and Richard Lewontin on his role in an attempted collective re-organization of a biology lab.

Finally, in a short review (a genre we hope to encourage) Bob Young analyses the implicit messages conveyed by the very form and authorship of E.O. Wilson's, *On Human Nature*.

Herbert Marcuse has died, and we wish to honour his passing. His work was of fundamental importance to the founding of the *Journal*.

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# DOWN WITH THE CONSUMPTION OF KNOWLEDGE:

## An Open Letter to Academics

**Mike Hales**

For this issue of *RSJ* I've written a review of Philip Kraft's *Programmers and Managers*. The review takes up a position on the politics of academic work which may strike you as quite unsympathetic and uncompromising (though I tried hard not to be personally aggressive). In this letter I thought I should try to show why I find it necessary to be critical of many radicals' work. In an attempt to be consistent with my own arguments, I'll do this by describing the activities I've been involved in, with shop stewards from ICI (Imperial Chemical Industries, Britain's Du Pont) and academics, over the past two years. In doing this, I hope I can avoid setting up what seems to be an abstract 'position'; for there are no abstract positions. There are only relatively detached *lives*, and even socialists lead their own.

### **The ICI Research Co-op**

In setting up the ICI Research Co-op, its members are trying to create an organisation with which to counter ICI's five-days-a-week, full-time, wage labour-based knowledge-producing system. A large proportion — something like 40% — of the staff of a multinational, technology-intensive firm like ICI are employed in producing the knowledge (theory and ideology together) that is required to keep the institution functioning. Given this, it seems fairly obvious that until workers' own organisations can approach this kind of effort (in person-hours of work, quality of information, fluency of organisation, range and immediacy of dissemination, and so on) there can be no role in the long run for labour in opposition to capital, except responding to capital's accomplished acts. To promote and sustain their own, positive strategy opposite capital, workers need to succeed in organising themselves for planning, and implementing plans. They need a severely practical grasp of the politics of knowledge.

This is one of the insights carried by the Lucas Aerospace stewards' corporate plan campaign<sup>1</sup>. The Lucas Plan was a development very much in my mind when I returned to ICI in 1974, after five years as a postgrad working in the philosophy and social studies of science. (I had previously worked for ICI for two years as a newly graduated chemical engineer.) During those five years I became involved in the local group of BSSRS (British Society for Social Responsibility in Science), booking visiting lecturers on 'science and society', and pressing (but hardly agitating) for this to be a topic in undergraduate courses. The BSSRS connection got me involved in organising a conference on Workers' Self-Management in Science, which in its turn led to the first *Radical Science Journal* collective and to casting my DPhil thesis in the form of a marxist critique of management science. The thesis (now completed as a 'labour-process' analysis of Operational Research) and the attempt to set up the ICI Research Co-op have occupied most of my time since leaving ICI for the second (and *last*) time two years ago.

The concept of workers' counter-knowledge production is still, needless to say, far from being worked out, and even further from being materialised in ICI in the form of the Research Co-op. But before I get on to the obstacles that stand in the way of this kind of development, I should describe what the Research Co-op currently is, and how it got there. Essentially it is a small group of ICI shop stewards (weekly and monthly-paid members of the ICI Combine Shop Stewards' Committee) and 'academics' (that is, non-ICI, non-industrial concept-producers) which produces the ICI shop stewards' newspaper, *Chemco News*, three times a year. The project was initiated by the academics. At the 1976 Conference of Socialist Economists (CSE), a number of us living and working in Manchester at the time were very impressed at what the Brighton Labour Process Group had been able to do by way of research on the political economy of a local firm (Creeds, a subsidiary of ITT). We looked for a connection to set up similar work. Since I was then working in the Management Services Department at the head office of ICI Organics Division, ICI was the obvious place. But in the time that it took us to get our ideas together, I had got to the point of resigning my job.

So, in the spring of 1977, I went (as a recent ex-employee and trade-union activist at Branch level) to the Combine Committee with a proposal for a 'co-operative research project', involving the Combine and some academics. The proposal was well received, and a couple of academics from the CSE ICI Study Group started to attend Combine meetings, feeling for a 'research' angle to the problems raised there by shop stewards. But it was another nine months before anything concrete was conceived to fit the bill. That was *Chemco News*. The three issues which have been produced in the succeeding year have been the product of a small but

expanding group of 'editors', linked with the general membership of the Combine through the Combine's regular meetings. The result, as yet, is hardly what any of us would call research, and it exacts a much more production-oriented commitment (at the level of time-economy) from the academics than we initially wanted to make. Nevertheless, the commitment on both sides seems to be sustainable, and the achievements are significant.

There are at least three aspects to what is being accomplished. First, *Chemco News* has contained a mixture of items dealing with current collective bargaining campaigns and demands and items which try a more analytical approach to broad issues (such as productivity bargaining, unmanning, recomposition of the workforce, investment). Second, the editorial work has been a joint effort of ICI workers and academics, and thus a material basis has been produced, in the shape of this collective practice, for future collaboration. Neither of these forms of interpenetration of theoretical production and 'non-theoretical' work existed before, at the level of ICI as a specific unit of capital (as distinct from the general theoretical level — *Labour Research*, for example — of the organisational level of the Combine itself).

But most important, there is now a medium through which ICI workers can raise issues, press campaigns, and seek information at a national (and to some extent international) level. Moreover, it is open to all sections of the workforce — as is the Combine itself — and independent of those trade union channels which tend to be policed to a large extent by corporate management and structured by the concepts and conditions of business interests; this is very marked in the case of ICI, with its many-tentacled Joint Consultation system. This is important as a basis for building the Combine as a worker-controlled organisation of ICI workers. The material fact of *Chemco News* is also necessary as a condition for any future research, properly so-called. Without such a means of communication and the organisation that it presupposes and signifies, there is little chance that knowledge of ICI, as politically-organised workers need to see it, could be based on sufficient information, or tuned usefully to workers' conscious interests, or propagated widely enough for theory to become integrated with practice on a significant scale.

These advances are still relatively insecure. Although the newspaper seems now to be established in terms of the mechanics of editing and printing, the next year or so will begin to show whether the potential for expanded organisation and expanded knowledge (the two are identical) will be realised. A lot of this has to lie with ICI stewards themselves, although to some extent academics' legwork and initiatives are still crucial and will continue to be so. For example, stewards still need to be prompted to think of *Chemco News* as a means of organising, along with

the picket line, the Branch resolution, the mass meeting, and so on. At the moment, a 'Chemco conference' is being organised, to co-ordinate action across ICI sites in the UK in pursuit of the 1979 Pay Claim and to discuss managements' attack on manning levels in various Divisions of ICI. Such a conference would not have taken place without *Chemco News* as an organ of the Combine.

### **'Think Work' and 'Non-Think' Work**

So, 'outsiders' have played a major part in the formation of the Research Co-op, and there are still major roles waiting to be filled from outside. We need more international links. We need more 'journalistic' labour-power and time. We need people to take up specific issues on a research basis (contract labour, international investment, multinationals' pricing and sourcing, computerisation of white-collar work, the dynamics of particular markets such as Fibres and low-tonnage organic chemicals), working with ICI stewards at various locations through the UK. But one point needs to be stressed. The crucial direction of the whole development is towards the socialisation of *ICI workers' own knowledge*, and the rigorous understanding and enhancement — by them — of its production.

To expand on this: as an ex-employee, it's fairly obvious to me that there isn't much that outsiders can add to the knowledge of ICI possessed by ICI workers, taken as a whole. In the course of a day's work an employee (especially in the white-collar sector), who keeps eyes and ears open and is tuned in to the problems of workers as opposed to the Company, can pick up many things which an outsider never gets drift of; and this knowledge can be supplemented by reading, say, *Labour Research*, *Chemical Age* and the *Financial Times*. The problem lies in that phrase 'as a whole'.

ICI workers are not a whole, in any significant material and political sense. There are geographical divisions, product divisions, labourprocess divisions, ideological divisions, hierarchical divisions. If you accept my premise about the need for a workers' 'knowledge economy' which is at least as vigorous and fluent as the Company's, then among the most vicious of the divisions are those between knowledge workers and others, and those within the mass of knowledge workers themselves: clerical and secretarial staff, computer workers, systems analysts, production planners, design workers, research and development workers, section leaders and junior production management. It seems to me that the complexity of this terrain — as an area for class struggle and political organising — is not well enough illuminated by discussion in terms such as 'the separation of mental and manual labour' (as we find it, for example, in Alfred Sohn-Rethel's work in marxist epistemology) or 'the separation

of conception and execution' (a slightly improved formula — but still a formula — used by Braverman in his critique of Taylorism in white-collar work). We have to be able to analyse this section of monopoly capital's collective labourer as a combination of levels and types of concept-producing and concept-reproducing practice, each with its own specificity of product and process. Workers' subordination as a class is reproduced by more than the formal and practical relations of wage-labour, commodity and private property. The structure of knowledge and non-knowledge that is reproduced by white-collar workers (via fragmentation, deskilling, hierarchy) seems to me to be one of the most urgent areas for marxist research and socialist political practice.

That paragraph carries hints of the theoretical challenge: to create a rigorous political economy of knowledge, which links with traditional perspectives that marxists have drawn from *Capital* (concerned with the structure and dynamics of exchange-value) while also drawing upon the more recent 'labour-process' focus. This means looking at the relations between use-value and exchange-value, and at the subordination of *workers*, in practices, rather than the subsumption of *labour* in capital. Academics can make some key contributions here to the necessary theory of revolutionary practice. But at the same time they need to do the most they can to materially link the analysis with practices that are aimed at *making* a whole out of capital's collective labour. The socialisation of labour accomplished by capital operates, crucially, through intersection of practices in *dead* labour — in detached products. In opposition to this merely objective socialisation, socialist socialisation has to be subjective; practices must determine each other through their intersection in living, concrete labour. To the extent that academics can theorise and practically attack the structures standing in the way of subjective socialisation of labour, they can begin to subvert the conservative consumption-politics of knowledge which neutralises so much 'radical' intellectual work.

As I was writing my review of *Programmers and Managers*, I read a review of some of the current major texts in the area of 'labour process' work: Braverman's book, the Brighton Labour Process Group's 1976 CSE conference paper, and the CSE's book of readings, *The Labour Process and Class Strategies*. The review — entitled 'The Romance of "Labour"' — was by Tony Cutler, and appeared in *Economy and Society*, Feb, 1978; and it drew my attention very sharply to the theoretical tangles that the current 'labour process' movement is implicated in. Cutler attacked the notion of 'tendency' which operates in, for example, the Brighton Group's paper where they attempt to describe 'immanent laws' of the capitalist labour process. Many of Cutler's targets turned out to be the same, empirically speaking, as those of my own review. If we take *practices* as the primary focus of analysis in labour-process theory,

it's obvious that there *are* relatively stable and historically significant structures in and of practices (e.g., 'the tendency of capital to deskill labour'). However, the conditions for the historical production and reproduction of these 'tendencies' may range from the very general (conditions of 'pure capital') to the very specific ('conjunctural'). We have to assume that, in principle, there are significant generalities at all these levels of abstraction. But what these generalities are, and how they relate to one another across levels of abstraction in the determination of specific practices, are questions whose difficulty and scope are still only being explored — to a large extent as a result of the labour process movement's efforts. The specific material and historical basis of its reproduction has to be clarified in every case where there seems to be a tendency in the capitalist labour process. Concrete analysis of this kind is urgently needed, without prejudging the ultimate status of conventional marxist concepts such as value, exploitation, surplus-value, and so on.

To this extent, Cutler's critique converges with mine, I think. However, I said that my targets were only 'empirically' the same as his. Because I don't make time to read Cutler's books and because I know little about his practice as an academic, I'm unable to assess in any depth the relation between his criticisms and my own; but reputation leads me to be rather worried by the fact that I find myself apparently on the same side of an argument as he is. The best that I can do by way of clarification at present is to stress this: my plea for a focus on *subordination*, as a level of analysis distinct from more conventional 'marxist problematics', is a tactical call. It is aimed at building on the imaginative, enthusiastic and — this is important — *struggle*-oriented movement that we've seen develop over the last couple of years around the rallying cries of 'deskilling'. I think that, in this, my thrust is quite distinct from Cutler's. I'm not looking to theory for internally-complete accounts of the *necessity* (in a logical sense) of relations of production in a social formation. I want us to be producing theory which illuminates the constraints on *possibility*, in an agitational context. What concerns me is not so much that 'capital' does not logically entail 'deskilling' (on that categorical issue I agree with Cutler), but that 'deskilling' is not an adequate conceptual weapon in the struggle to conceive of and create socialism.

'Linking theory with practice' is the formula that we tend to use to label the problem which now appears central. However, in that form it doesn't adequately highlight the breaking-down of divisions between production and use that is necessary. It would be more adequate (but less elegant) if recast as: linking theory-producing *practices* with practices in which creative thinking is suppressed. Interpreted in that material sense, it is not an easy formula to work to. When I re-left university and re-joined industry, I went armed with theory. As it happens, that theory (derived largely from Habermas's revision of marxism, as translated into English

in the late 60's) was shockingly inadequate; but that is not the point. I was very sensitive — as it turned out, hypersensitive — to the need to 'link theory with practice'. But two and a half years later I quit the job. Coming all together, the strains were too much: rank-and-file trade union activism, serious creative theoretical and ideological work, 'personal' politics as a father and husband, and wage-labour itself. And what about just 'being', with friends; having a good time without being intent about it? Forget it!

The theoretical work got no support in the immediate (wage-) labour process, or in the trade-union practice closely associated with it. There was little purchase for personal politics in the snatched businesslike (yes!) interludes of trade union branch work or in the infrequent discussions that I could fit in with academic comrades. Wage-labour imposed a structure on time and energy which immediately imposed a handicap on any attempt to pull together any other practice composed out of the excluded fragments of life. 'Work', 'life', 'politics' and 'theory' tended thus relentlessly to separate one from another, despite self-conscious attempts to hold them together within the limited but relatively manageable space of an individual's practice. Hardly surprising? Maybe. But somehow, some people — some individuals — have to be enabled to bring it off. Or else we write off prefigurative struggle in the 'productive' organisations of monopoly capital. It is with this enabling — each for the others — that a socialist politics of production has to concern itself.

### ***RSJ* and Socialist Politics of Knowledge-Production**

Maybe now you can see how my criticism of 'detached partisanship' relates to my practice as a marxist intellectual employed in industry. (And — here's the catch — to my subsequent practice as an unemployed marxist intellectual: much more productive in political, personal and theoretical senses — but not capital's!) My criticism is not an attack on academics for being academics, rather than employed directly by capital, or unemployed; or for writing books; or for responding to the contradictions of life and work as a socialist and an academic with various rationalisations, sublimations, projections and repressions. Having failed (painfully) to cope with my own contradictory status as a wage-labourer, I'm the last one to criticise unsympathetically on this ground. But as someone with one foot (or a few toes, at least) in industrial trade union practice, what I'm looking for is *practical* convergence with academics' work. What I mostly see around (and am thankful for) in contexts like *RSJ* or the CSE is abstractly ideological (or even theoretical) support. But that is not the same thing as the intersection of socialist intellectuals' practices in *living* labour, and it can neither substitute for it nor, of itself, generate it.

That's why I want to invite you to extend the scope of *RSJ*. Its content, too often, has the (academically-conventional) form of disembodied end-product; the dead labour of writing radicals, socialists, marxists, intellectuals. I would like to see it include more about what it is that we're trying to do, and where we need support, and what obstacles we face in pulling it together. I would like to see it used by readers to make working links with others. This letter, and the review of Philip Kraft's book, were written, not as academic knife-fighting or abstract philosophising (and I hope they will not attract those responses), but as invitations to help and as encouragements to re-group. You can read this as an appeal for time and effort on the ICI research front if you like, although that hasn't been my main intention.

At times, especially when I was working in industry in a provincial city, I've found it very difficult to draw together *RSJ*'s manifest interests within the space of my own commitments. I have heard others, who have (i.e., make) less time to read and write theory, talk about *RSJ* as Left-intellectual wanking. That description is a gross misrepresentation of the work which lies behind the printed page. But, even as a relative insider, I have at times needed more effort and goodwill than I could muster, in order to see through the product-form contents and reach the commitment that they embody. A way to avoid this problem would be to reduce the scope; that way it could span a small-enough spectrum of practical commitments for readers to know implicitly what writers were up to. This is how non-radical science journals work. But finding a way actually to *tackle* the problem may possibly succeed — now, nine years after the first stirrings of what eventually became *RSJ*. We could try not to continue so much in the vein of theory as critique (re-speaking, in a more materially-connected way, what is conventionally spoken in other, non-radical, practices.) We could be trying to produce contributions that deal with the theory of socialist practice — of *organising* — in knowledge-producing work.

This shift is only a possibility. Its realisation depends on the practices of the *RSJ* collective's members and of the readers of *RSJ*. I know that this matter exercises the minds and efforts of the collective. A great deal lies with you, readers. What are you working on; what practices, what organisations are you trying to build? What innovations in Left politics, trade union practice, 'science', academic production, are you engaged upon or looking for? Where are you seeking connections, and where are you hoping to see fruits from your efforts over five, ten, twenty years? In short: What is our practice? Unless we deliberately go about sharing our experience and ambitions, and thus get the measure of our collective practice as 'radical' academics or scientists, how can theory be theory of practice? And until we have a theory of the politics of our practice as

knowledge-producers, how can a radical science movement hope to be coherent or effective?

In anticipation and solidarity,

Your hectoring comrade,

Mike Hales

## NOTE

1. For some details of the stewards' campaign, see Mike Cooley, 'Lucas' Socially Useful Prototypes', *Undercurrents*, 26, 28-31 and Dave Elliott, 'The Lucas Aerospace Shop Stewards' Alternative Corporate Plan', in *Trade Unions, Technology and the Environment*, Milton Keynes, Open University Press, 1978. The Lucas Aerospace shop stewards' combine has set up a joint venture with academics via the North East London Polytechnic: The Centre for Alternative Industrial and Technological Systems (CAITS). For further information contact Mike George, Co-ordinator, CAITS, North East London Polytechnic, Longbridge Road, Dagenham, Essex, RM8 2AS, England; phone 01-599 5141, 01-597 4630.

## In issue 42

The Other Einstein  
Workers Control of Technology  
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two years \$26  
Sustaining subscription \$30  
Foreign subscription \$16  
Back issues \$3

New Fronts Publishing Company  
4228 Telegraph Av. · Oakland, Ca. 94609

# SINISTER MEDICINE?

## A Critique of Left Approaches to Medicine

**Karl Figlio**

### **Staying on the Ground**

Brian Hurwitz, a junior hospital doctor in London and a member of the *Radical Science Journal* collective, recently gave an *RSJ* seminar which he later described as 'a necessary regression'. I interpret his comment to mean that his own unity as a person and his sense of collective integrity with the rest of us demanded an urgent statement of just what the day-to-day job of hospital doctoring was like. It was one thing constantly to critique, but quite another to face the overwhelming imperative both of human suffering and of the institutionalized way of seeing it and responding to it. I suspect also that his account, which pointedly under-emphasized the theorizing of experience which we had expected and which pressed home to us the inexorableness of modern medicine's course, projected an anger onto the collective which he had previously turned inward in the form of a denunciation of his own inadequacy.

I'll come back to Brian's account in a moment, but first I want to say why I've brought it up at all. As both a qualified doctor and a long-standing member of the *RSJ* collective, Brian lives a split which recurs over and over when science and medicine are criticized. In the case of medicine, the seeming inevitability and the obvious benefits of scientific progress are driven home more urgently by the demands of human suffering. That immediacy obfuscates the conventional nature of the usual medical response and pushes aside the social critique of medical practice as irrelevant to that moment. And to the extent that these moments of acute crisis and conventional medical response give a concrete location to our fantasies about suffering and dying, they remain an area more of lurid fascination than of social criticism. Any new practice would seem to be as utopian as either not to die, or to die free of the fears condensed into the fantasies we normally have about dying. That's just how compelling existing practices are. As hospital medicine comes increasingly to define

medicine itself, so that the hospital becomes a metaphor for the social relations of medical practice in all its forms, the schism between social criticism and medical reality becomes seemingly unbridgable. That split is certainly acute in a practising physician, but it is just as certainly a part of any of us whose political consciousness focuses upon science and medicine. And more importantly for left strategies, it is just as certainly there when different people or groups occupy opposite poles of that split.

I am trying in the first instance to sympathize with the peculiar dilemma that doctors feel when placing their practice in a theoretical and political context. But, in pointing out that their position is only a more acute form of what anyone living in our society must feel in trying to bring science and medicine into a political arena, I also want to argue that we must learn to face the intermediate areas between living in untheorized experience and thinking in coherent political discourse. When we don't face that level, we slip into either a heady belief that medicine will sort itself out with a change of staff in political structures at large, or that the problems are intractable beyond all hope of change, embedded perhaps in human evolution. The former position collapses medicine into medical personnel and medical personnel into the political hierarchy of the capitalist state; the latter collapses medicine into disease and disease into human adaption to nature.

Fourteen years ago, in one of the *New Left Review's* rare excursions into science and medicine, Martin Rossdale wavered around this dilemma — the fact that it's still with us in our critical thinking shows just how difficult the resolution will be. He posed the problem of medical change in terms of social vs. curative medicine, arguing the need for the former. As an example of social medicine and its potential impact, he gave the case of smoking as a health hazard caught up in the capitalist dilemma; it's bad for health, but profit-making for industry. It's pushed by advertising, and it's a source both of tax revenue *and* of concern for the state. What can the doctor do, not only in the midst of this tangle of economic and political interests, but also from within a trap of class position which makes a concern for, and communication with, a large number of patients difficult? Social medicine suggests a new way of looking at health and disease — less emphasis on the acute diseases that bring people into the health-care industry, and more on the reorganization of society in a healthful form. But 'conflicts will remain [between health and medicine] so long as health is considered a commodity and the patient a vehicle which brings disease to the doctor (Rossdale, 1966, p.9).'

I agree that commoditization of medicine is an important part of the problem, and also that class stratification restricts even further the chances that doctors might successfully oppose the pathogenicity of capitalist society. The fact the Rossdale exemplifies his alternative to curative

medicine with cigarette smoking brings out the dilemma for the practitioner clearly. The imperatives of medical practice prevent doctors from imagining a socialist medicine or a socialist concept of disease. What they *can* do is intervene in the 'natural' history of some diseases. As a result, social medicine stands in for the socialist medicine, and social medicine comes to mean preventive medicine — a medicine wholly within the framework of medicine as it already is, but with a greater emphasis on convincing people not to do what medicine has discovered to cause disease. Faced with what to do, the doctor says, 'Don't be careless.'

There's a seduction in this style of argument. It's understandable and even necessary, but it's still a seduction. It says that there are genuine problems which are not being countered by medicine. They're not airy-fairy; they're down-to-earth, concrete, socially perpetuated pathologies: smoking, malnutrition, pollutants, industrial hazards, misallocation of resources, sex and race discrimination, food additives. Socialist action means opposing these outrages wherever they occur. I agree, and I approve of groups like Science of the People in America, and BSSRS and the Politics of Health Group in Britain. I support hospital doctors who meet the crises as they come to them, and look for new ways to make their responses meet human needs. But there is also a pre-emptive stridency in this position, one which implies either that this kind of practice *is* prefigurative, e.g., that discouraging smoking *is* an example of socialist medicine, or that the distance between curative medicine and socialist medicine is actually insurmountable, so that their practice remains as the only engaged alternative.

It is because this work is so useful that it is difficult to acknowledge that it is also a way of living within the contradiction that medicine both relieves suffering *and* prevents our seeing the pathogenicity of capitalist society, (e.g., that how we are is related to how we are sick). That contradiction is built into the socialization of medical personnel, the organization of health care and the perception of illness itself. Marc Renaud put the latter aspect this way:

'This view of health and illness is congruent with the larger capitalist environment because it commodifies health needs and legitimates this commodification. It transforms the potentially explosive social problems that are diseases and death into discrete and isolable commodities that can be incorporated into the capitalist organization of the economy in the same way as any other commodity on the economic market.

With such a paradigm, "society" is epistemologically eliminated as an important element of the etiology\* of disease, therefore impeding the growth of a consciousness of the harmfulness of economic growth (Renaud, 1978, p. 109).'

That epistemological silence is experienced as an acute contradiction by

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\* The study of the causes of disease.

the doctor who questions his/her own medical practice, so that in the face of stark alternatives between socialist and capitalist medicine (perhaps only an imaginary alternative at the moment), they are likely either to retreat into their accustomed practice or to look for areas which lie within medicine as it is, but which at least point in the right direction.

I'd like to get back to Brian's talk. He gave a vivid account of the symbolic language and action surrounding a patient coming into hospital. It's at this moment that the socialization of medical personnel, the human context of illness, the socio-economic structure of health care and the social construction of the very perception of disease are brought into play. Of course, it's also at this moment that prefigurative action, or the realization that the hospital is partly a metaphor — an abstract cockpit — for medical practices, technologies and instructions which, as 'frozen social relations', can be deconstituted and reconstituted, is least likely to occur (Bob Young, 1979 and forthcoming; London, p.105). It brings home what we're dealing with:

Not only is a disease tracked down like a dangerous criminal in the body, but open war is declared by the medical establishment on certain diseases, especially cancer. And there is a set of medical terminologies, usually slang terms, which faithfully reflect this. The cytotoxic chemotherapy that's used with patients with cancer is literally referred to as poison. They actually talk about poisoning patients ... When one gives injections to sedate people, to put people to sleep or to give them anaesthetics, one actually talks of slugging patients. One talks of killing pain and, in cytotoxic language, there is a very important concept known as tumour-kill, which is from the language of nuclear war.

One has to somehow devise ways of containing the really extreme violence that's going on, and the aggression that's implied in the various forms of treatment, whether it's operative or whether it's chemical, and different people have different ways of doing it. Some doctors retreat and actually lose themselves in a compassionate, tender, caring role, and somehow become shells of people; they become merely a role.

Within the context of practice, the point about the patient arriving in hospital is that, without actually being aware of it, the patient arrives and in a sense finds himself or herself in the middle of a war — really, a sort of frantic battle. And in some sense, this battle is going on independently of the patient. The struggle is really between doctors and disease; the patient's a sort of vehicle for the fact that the disease exists ....<sup>1</sup>

No literature which fails to speak to this level is adequate, because that silence robs the argument of an entry-point for practice and a recognition of the compelling grounds for resisting change. I don't mean by this level just the dramatic moment which disarms all critiques by the need to overcome a crisis. I mean what is acted out at that moment, seen as the focusing of social, economic, physical and symbolic processes, whose momentum makes even the first step of an alternative practice inconceivable.

I don't mean categorically to exclude all positions which do not have an obvious practice built into them. We must have theory and we must have continual criticism. What I want to explore are the ways in which political

and humanistic analyses of medicine often preclude seeing either how an alternative practice might begin or how to hold to a socialist vision in the midst of struggling to create progressive practices. My point is simple to make, though difficult to realize. Theoretical writing must help to open up spaces for alternative vision in which such struggles take place.

### A Catalogue

My commission to write this article was, in part, to survey the critical literature on medicine, especially that of the left. But I realised as I got underway both that I intended to write in a more personal and thematic vein, and that, in the meantime, critical sweeps of the literature and political positions had recently appeared. What survey remains in my project will be included in the thematically structured article, rather than separated out. But first, let me at least organize some of this literature into clusters; then I will come back to my point about making socialist theory part of opening up spaces of alternative vision:

1. Recent overviews: John Ehrenreich, Howard Waitzkin.
2. Liberal bio-medical: René Dubos, John Powles, Thomas McKeown.
3. Radical: John Ehrenreich, British Society for Social Responsibility in Science, *Science for the People*, Radical Statistics Health Group, Politics of Health Group, Ivan Illich, Health/PAC, *International Journal of Health Services*.
4. Feminist: Barbara Ehrenreich, Deirdre English, The Boston Women's Health Book Collective, Mary Hartman and Lois Banner, Sara Delamont and Lorna Duffin.
5. Marxist: Marxists in Medicine, Vicente Navarro, Michael Schneider, Hans-Ulrich-Deppe and Michael Regus, Ronald Frankenberg, Howard Waitzkin and Barbara Waterman, Julian Tudor Hart, Union of Radical Political Economists, Health Movement Organization.
6. Academic: Medical sociology, medical- and bio-ethics, social history of medicine, epidemiology and community medicine.
7. Trade union and practical: Patrick Kinnersly, *Hospital Worker*, NUPE, Fightback.

This is meant to be only the roughest sketch of broad orientations of people, groups and publications, and it mainly refers to literature cited in the Critical Bibliography. I don't claim to have characterized the diversity

of activities in which they are engaged. So the list is partly a heuristic classification and partly a way of referring to a bibliography which readers might find useful.

I had originally intended to work through these groupings one-by-one, in line with my commission. But, taking the more thematic course I mentioned above, I shall instead draw out some crucial similarities and differences which cut across sectors of critical writing in unexpected ways.

I'm aware that both this list and the rest of my paper leave some very important movements either unmentioned or inadequately examined. The women's movement is one of the victims. It has provided a continuing impetus to the struggles to restructure medicine at a theoretical and historical level, at the level of organized parliamentary and alternative activity, and at the level of self-help and left anti-authoritarianism (abortion reform, the Women's Therapy Centre in London, the Boston Women's Health Book Collective, *Spare Rib*, self-examination and womens' health groups, including the expansion of the campaign to save the Elizabeth Garrett Anderson Hospital — a women's hospital staffed by women — to include a Well-woman Clinic). John Ehrenreich and Howard Waitzkin would put the Black and Puerto-Rican health-care struggles in America next to the womens' movement as part of a continuum of radical anti-authoritarian movements. Waitzkin says:

'A network of alternative health programs has emerged that tries to develop self-care and non-hierarchical, anti-capitalist forms of practice; these ventures then would provide models of progressive health work when future political change permits their wider acceptance (1979a, p. 41).'

I realize that I say nothing more about these radical alternative or anti-authoritarian movements. And I'm almost as silent on trade unionism. My paper has taken a different direction, partly because of my own experience, which includes them only second-hand and which has accustomed me to working with literatures more than with other practices, partly because of a belief that theoretical and historical work help us to open a space of alternative practice, and partly because of a desire to understand where our socialist vision falters. I think it's also true that radical movements, including the womens' movement and trade unionism, have been written about. I've tried to compensate for my own failure to do so by expanding on that literature, and on the authors in the catalogue above, in the references. This enlarged reference section forms part I of the *RSJ* Critical Bibliography.

### **Posing Intractable Alternatives**

I'm going to put next to each other a set of short quotations from authors

whose apparent political positions and critical grasp of medicine are quite different. I'm trying to speak to the point I set out with — putting our theory in the service of socialist practice by helping to open up alternative spaces:

1. 'Medical problems posed by the environmental stimuli and insults of modern civilization have acquired a critical urgency... Until recently the rate of change was generally so slow as to allow time to make the proper conscious or unconscious adjustments .... Now [it] is so rapid that there may not be time for the orderly and successful operation of these ... adaptive processes (Dubos, 1970, pp. 125-6).'
2. 'The social relations that bound people together in agricultural societies were shattered by the development of commodity production under competitive capitalism. The competition demands continuous improvements in efficiency and productivity. These are achieved with a "flexible" labor force, a fast pace, and relentless technological change ... The most human solution [to the problem of stress-related mortality], and in the long run the only real one, is to halt the social disruption and recreate relaxed community (Eyer and Sterling, pp. 16, 35).'
3. 'The root of the ideas contributed by capitalism is that of disease as a commodity, and I feel that the aims and aspirations in health before the rise of the industrial revolution have been perverted and twisted by the needs of that sort of economy (Robson, 1975, p. 24).'

Variations on the theme of 'community' run through these statements. Once, there was a community; the modern industrial age has disrupted it, and the loss is marked by disease. It might seem unfair for me to put together people of such differing political persuasion as Robson, a regular contributor to the British Communist Party's *Medicine in Society*, and Dubos, an eminent biologist who writes as a liberal humanist. Indeed, I shall point out how different their proposed practices are. But first I want to elaborate on each of these quotations, in order to bring out the similarities embodied in the shared theme of community.

### Dubos and the Liberals

Dubos anchors his analysis of the diseases of modern society, and the potential responses to them, in the long and gradual evolution of human nature. Adaptation is the source of health — balance and harmony refined over ages of accommodation to a stable environment. Industrial society (not capitalist society) has wrenched that relationship out of balance. Dubos trusts the power of science to solve all problems, including this one. The limitation is one of insufficient scientific resources — a problem of 'values' (commitment), not of science itself.

We can see the theme of community in the notion of an adaptation of human nature to external nature undistorted by human intervention. By a curious inversion, however, that human intervention, carried along by

the rise of industrial society (and in accordance with human nature?), becomes more indomitable than nature itself. The problem, for Dubos, is to understand nature and human nature fast enough to re-establish continually the balances between them which are necessary for health.

Dubos' position is positivistic in the extreme; science is about nature; values are about culture; the two do not mix, unless the knowledge of human nature might help us make wise choices. The result is that the whole burden of facing the consequences of socio-economic and political forces is put onto the scientific investigation of nature. Human beings are to develop by changing nature, not by changing their politics. And yet, because of his positivism, he appears to be advertising the humility of science by granting that all choices are issues of values, not of science.

It is also utopian, in that it combines the identification of health with community now lost and with an unending scientific struggle to build ever newer ones in the wake of a changing civilisation. On one side is external nature, indeed, the enormity of millions of years of evolution; on the other is human nature. Both are great unknowns, whose mysterious harmony is constantly ruptured by civilization — an untextured, universal property of evolving human nature. Humankind will regain its lost health (yet another universal category, not in having an absolute definition, but in having no historical specificity beyond a kind of fashion) in that land of harmonious unknowns. Science, not social practice, and not science as part of an integrated social practice, will bring it back from that land.

Dubos' work belongs with a cluster of bio-medical criticisms from the 'experts'. John Powles and Thomas McKeown belong with him, although their messages are somewhat different. They are interested in the relationship between medicine and the improvement in health of already industrialized countries in the late 19th century. By historical-epidemiological studies, they show that (as Dubos also pointed out) medicine contributed little to that improvement. McKeown works through the possible causes for it, and finally settles on improved nutrition.

To say that medicine had nothing to do with humankind's most significant health achievement since industrialization is implicitly to shift the category boundaries, so that nutrition is moved out of medicine's jurisdiction. McKeown himself says this (p.167), but, nonetheless the overall impact of his analysis is to appear to dismiss medicine as a force for health. In the process, he has become a figurehead for the fashion of disparaging medicine. I'm concerned with how co-optable his authority is. And resting as it does on a definition of medicine as 'science', and not on socio-economic and political relationships of health and disease mediated by medicine, his medical humility can easily become another variant

of Dubos' scientific orthodoxy.

As I did in the case of Dubos, I would call McKeown's hopes for health utopian, not in the sense of fanciful (for their specified goals are as apparently realistic as any others), but in the sense of lying outside human social practice. The answers lie on the other side of a cleavage between humanity and nature, to be brought back from that other side by experts. Human beings don't change through social practice; selected emissaries go to the other side to negotiate a treaty on which human health depends. There may be, as there was with Dubos, a myth of a primordial community between humankind and nature, which envelops and legitimates the entire discourse on health. I'm calling that style of perceiving the essence of health, steeped in a dualistic split of people from nature and in the expert mediation between them and the source of hope in nature, a utopian position.\*

### **Eyer and Sterling — Radical Humanism**

It may seem a bit strange to put next to Dubos and the liberal medical critics someone who works with the American marxist medical group, Health Movement Organization (HMO), and who writes for the Union of Radical Political Economists. Joseph Eyer and Peter Sterling extended HMO's notion of a 'materialist epidemiology' into a concrete study of mortality from stress-related diseases in capitalist societies. A materialist epidemiology means an analysis of the causes of diseases as social, rather than individual, events. Eyer and Sterling begin by showing that the periodic economic fluctuations in capitalist societies are accompanied by synchronized fluctuations in mortality. But, contrary to our expectation that low economic activity and employment levels mean higher mortality rates, the converse is true; mortality levels correlate with high levels of economic activity — with capital at work, not in recession.† Let me go on a bit more about their historical materialist epidemiology before coming back to their relationship to Dubos and the others.

Eyer and Sterling try to specify the conditions for increased mortality by using both historical mortality figures and figures from other cultures to

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\* It has been pointed out that this is an idiosyncratic sense of 'utopian', which usually connotes future, where some of my examples might better be described as 'nostalgic' or 'golden age' or 'romantic' or 'natural state' (à la Rousseau). Of course, as ideals, these images of the past refer to the future as well. The sense I want to evoke also has overtones of 'idealist' and 'a-historical' thinking. I really mean unmarxist.

† I'm not interested in testing the validity of this statement, but rather in analyzing the political meaning of their argument. It is part of a largely American epidemiological debate. See Brenner, Draper.

establish a comparison between capitalist and non-capitalist societies. They found that mortality rates increase smoothly with age in non-capitalist societies, but that, in capitalist societies, mortality rates increase sharply with age around late adolescence, before dropping back again to a gradual and slower increment with age. They take this notch in the curve around the time of entry to the labour market to indicate the specific effect of work under capitalism. They also compare the relationship between blood pressure and age among different types of cultures, from capitalist (urbanized or rural) areas, to disrupted traditional agricultural communities, to undisrupted hunter-gatherer societies. The increase of blood pressure with age which we take to be normal (it was in my physiology text at university), and connected with the expected degenerative diseases unmasked by the older age distribution of modern western society, is less steep in the disrupted agricultural communities and non-existent in the hunter-gatherers. They try to put these suggestive observations of specifically capitalist mortality together with the accumulating evidence relating stress to illness, especially that on the stress of social and personal disruption.

It's easy to pick at isolated points of their argument, such as the possibility that the higher mortality of boom periods develops from a lagged effect of the previous depression, or that their cultural types are not carefully enough specified, or that the effects called capitalist are really urban or industrial. But an overall shape — a way into the problem of socially specific pathology — does emerge. And it invites us to respond in socio-economic and political terms, not just in the language of medicine. Recasting previously medical issues as political ones does raise eyebrows. It is important not to allow such a restructuring to appear simply as a category mistake, but to include it as part of an overall strategy to reconstitute knowledge within a socialist framework — one which sees where capitalist knowledge won't look, and illuminates relationships it won't acknowledge.

In trying to draw together the extremes of large-scale capitalist phenomena at one end and disease at the other, Eyer and Sterling have had to introduce — as anyone would — likely mediating links, such as 'stress', which seem both to depend on capital and to affect pathology. Those choices are not refutable by medicine, at least not by medicine on its own, but form the bases for a continuing political debate which will set new questions with medical and social aspects.

I'm dwelling on Eyer and Sterling's epidemiology, and the terms of discussion, because I sympathize with their project. But I also see the possibilities for easy co-option by contemporary medicine and I disagree with the particular mediations they've chosen. As the quotation above indicates, they take the mediating factors between capitalism and disease

to be the moulding of competitive, striving people and the disruption of stable communities. Capitalism engenders chronic stress by preventing relaxed communities from reforming, and by making people unfit to form them even if there were the opportunity.

They see hope in the growing number of relaxation methods, in intimate communities, such as encounter groups, in family and network therapy. All these have limitations, they say, in that they are movements so much within capitalism that they get caught in the contradictions, e.g., 'finding that "space" between the objectively necessary relationships of modern society, and the actual social situations of people with extreme problems' and then trying to open spaces for large numbers of people (p.36). Genuine community formation will ultimately endanger capital accumulation. The experience of the Berkeley student psychiatric facility during the free speech movement of the '60s illustrates their idea of the community-forming process. For a decade before, the proportion of students treated had been rising from the original 8-10%. During the rebellion, the number fell drastically; with the defeat of the movement, it rose again to 'normal' levels. This process contrasted sharply with that at a preventative psychiatry facility in Manhattan, where rates rose and cures were no more than the spontaneous rate of improvement. Genuine community formation in one case; therapy without it in the other.

As I said, I sympathize wholeheartedly with the desire for spontaneous communities, and with the promise that they might teach us to live humanely and healthily. Only a cynic would dismiss the hope and the political impact of the '60s communitarian drive. But we must also not forget that their prime example was not only a moment from the '60s, but was also at one with the anti-authoritarian spontaneous uprisings of that time. That is a difficult revolution to sustain, especially if it aims to conquer capital accumulation. Indeed, in America, capital provided once again the outrages to galvanize revolt (Vietnam, Watts, the suppression of the Black Panthers) and it provided it in mouthfuls too big to chew; the only way not to choke was to spit them out hard.

Eyer and Sterling are aware of these limitations, and they argue that medical science should put its energies into exploring the effect that mass uprisings demonstrably have on reducing stress, looking to extend its practical application. Still, we're back with spontaneous community formation in the wake of outrages whose specifically capitalist organization is unclear. There isn't any practice here, because the nucleus for change is spontaneous rebellion; what practice there is comes from medicine, which will analyse the impact of uprisings on stress.<sup>2</sup>

This is the clearest counter-hegemonic argument I know of, which con-

vincingly brings health and disease into relation with political change; and yet it is inadequate at a crucial point. The struggle for political change and the struggle for health are not only divorced from each other, they essentially do not exist themselves as practices. Health remains an unknown effect of rebellion, which remains an unknown effect of capitalist outrages. But the ill-health which that rebellion must overcome is an effect, not of overt suppression, but of countless daily intrusions (Berliner and Salmon).

Surprisingly, I came across the following statement from establishment medical circles: 'A number of studies have identified factors *intrinsic to the job* which have been found to be stressors linked to CHD (Cooper and Crump, p.420. CHD = coronary heart disease. The emphasis is theirs).' Of course, the very form of presentation neatly isolates elements from each other. Work doesn't cause disease; it contains *factors* which, in turn, have been found to be *stressors*, which, in turn, have been *linked* to CHD. All the components which we would like to grasp in one perception of capitalism's pathological expression are separated by conceptual walls. The recognition of the problem doesn't even arise, and serious medical work on stressors can continue. Nonetheless, they have organized our perception in one crucial area more clearly than Eyer and Stirling; work itself, not merely an environment incidental to the work, is *intrinsically* pathogenic.

This is stronger than they would have put it, but the seeds are there. Of course, for us it is important to see 'work' as itself socially constituted, rather than to distinguish it from a social environment. Their formulation, nonetheless, prevents the attribution of occupational illness to merely external trappings of the labour process.

This gets me back to spontaneous community formation vs. practice. Eyer and Sterling have in mind as a presupposition the prior existence and the desirability of natural communities. Capitalism disrupts them and blocks the human urge to re-form them. It does this to sustain a mobilized, 'free' labour force. From capital's point of view, communities bind people, so that they will neither move wherever they are needed, nor aspire to succeed as isolated individuals pursuing capital's ideal of competitive achievement. From the side of the community, this binding is bonding, and capital's free labour is slavery. Community and capital accumulation cannot survive together. In their analysis, the tension between these two forces causes stress, which causes illness. So there is an implicit functionalism with a utopian backdrop. No matter how weak one of these two forces may be against the other, the organizing principle is a balance between formation and disruption of an entity whose existence is conceptually necessary. The whole structure of the analysis depends

upon positing it, even though it is more an object of nostalgic reverie than it is one that people have ever seen.

The idea of spontaneous community formation and capitalist disruption precludes specific social practices grouped around concrete areas of struggle, such as work. Maybe 'preclude' isn't the right word. I don't mean that these two modes of action exclude each other, but rather that putting community between capital and disease reconstitutes potential collective action within specific capitalist domains in an abstract form, in which two sets of forces oppose each other, oscillating — with a strong bias at the moment — around a steady state called community. These forces operate outside the organized labour market where capital accumulation occurs. They actuate a cataclysmic contest between two universal abstract principles, capitalism and community formation. And in the midst of that dramatic opposition between evil and utopia, we do not formulate specific practices with respect either to humane living now or to the global struggle against capitalism.

I feel uneasy coming down so heavily on their position, because I share similar ideal. I also accept their analysis of the way capital fragments communities in the process of 'freeing' labour. So I grant the strength of their analysis, at least in part. But I want to distinguish between community, which I can only idealize, and collectivity, which I propose as a basis for change through specific practices. Community is an abstract and universal ideal type, and, therefore, a geographical fetish — a localized embodiment of virtue; collectivity is necessarily concrete and local. Community stands outside any characteristics of capitalism except the need to disrupt them; collectivity is shared action within the several locations of conflict within capitalist society. Community, as an organ for change, is reactive; collectivity is creative.

Because of its abstractness as a natural attribute of any locality, community also invites cooption. The authors of the quotation above about disease and work would surely agree that work, maybe even some factors associated with capitalism, may injure human aspirations for a better life. Dubos could become quite eloquent about science and values, telling us that science is ready to solve any technical problems that people choose to pose. The choice and the posing would remain, for him, in the free domain of human values. Science, in characteristically positivistic humility, would have nothing to say about values; it is only a neutral tool. Capitalism, he would continue, has indeed posed severe problems for the human community, but that's an issue of values until that community chooses to put specific technical questions to medicine, in the hope of understanding nature and human nature enough to mitigate the consequences. Within this framework, medicine can continue to approve of community and to investigate the factors underlying CHD without ever

contributing to the reconstitution of the problem as a whole. Indeed, it would certainly keep the pieces fragmented and block any holistic perception of capitalistic pathology. The problem would never arise.

Eyer and Sterling have also brought into a focus a problem which I only want to raise — I don't have an answer, nor do I know if there is *an* answer. They attack the capitalist ideology of 'what's good for General Motors is good for the country' by attacking the science that comes forward in its support. So far, so good. But what then happens is that they offer oppositional scientific arguments — a kind of counter-epidemiology — as the vehicle for their political critique. Bourgeois epidemiology is wrong to stress the damage of unemployment, they say. We can prove that business kills.

Berliner and Salmon, who, along with many others in the American health-care left, clearly admire Joseph Eyer and follow a similar route, draw the same kind of counter-scientific battle line. They trace the historical trend from acute to chronic disease to make the case that multifactorial (implying environmental) causes count more in modern society. They also reject the physiology in which the body is a system in equilibrium — a neo-classical economic organism. Homeostasis must be replaced by a 'fight against intrusion'. 'While scientific medicine views people as unhealthy when they deviate from a statistical equilibrium, we see people as unhealthy when subjected to social assault(p.47).'

They are right to try to understand the ways our bodies reflect in their illnesses the internalized conflicts of society. In that sense, disease is a political act, only we usually don't know it. And if we do know, we don't know how. They are also right to say that our scientific concepts — epidemiological or physiological in these cases — transpose to our bodies a framework which derives from, and then reinforces, our theories of socio-economic structure. But I get nervous — not dismissive, but genuinely unsure — when they pose their political opposition in the form of a counter-science. It's as if they have seen through the science to the politics of the establishment, only to allow that establishment mode of discourse to set the terms of debate. I *do* think that we need to develop counter-sciences. Bob Young has even argued for a journal for radical research. I just want us to be clear about who's setting the terms.

### **Robson and Marxist Determinism**

I want to turn now to the last of the three quotations — the one by John Robson of Marxists in Medicine, a group within the Communist Party of Great Britain. Though perhaps unfairly, I'm taking his position as an

illustration of a more general one. One gets a sense of it, browsing through their journal, *Medicine in Society*, as well as in the basically political economic critiques of medicine, such as those of Vicente Navarro (I'll come back to Navarro). For Robson, health/disease took on its mechanistic, specific-etiology formulation under capitalism. Capitalism makes everything into a commodity, including its health-care system and its conception of disease. One finds statements in this genre which call health itself a commodity; Robson certainly considers disease to be a commodity under capitalism (See the quotation above). One could elaborate on this, and say that the pieces that medicine deals with, whether they be concepts of isolated pathologies or isolated functions of the parts of the body, or the material realizations of isolated physiology and pathology in machinery (such as heart-lung machines, dialysis machines and spare-part surgery) or, finally, the structure of health care itself, with its splitting up of responsibility for health into fragments located in different labour processes, are pieces of a capitalistic system of value and exchange.

At first glance, this analysis offers quite a lot. It puts the structure of health care firmly within the capitalist system, so that the critique of medicine can become part of the critique of capitalism, and the struggle to change it can become part of the transformation of capitalism to socialism. It also introduces, conceptually at least, a level of mediation between the political economic structure and the practice of medicine; modern theories of health and disease spring from the same soil as do the structures in which we live, work and receive medical treatment.

There remain, however, strong hints of a utopia, even within this ordinarily down-to-earth literature. The most obvious clue comes from the Robson quotation, in which he speaks of the commodity formulation of disease as a distortion effected by capitalism and introduced through the hospital-based medicine of the modern industrial period. 'Distortion' always presupposes an undistorted state, and Robson goes on to specify that precapitalist 'aims and aspirations in health' characterized this natural state which capitalism 'perverted'. Just as 'evolution' or 'community' underlay the health of pre-industrial society in the critical positions above, 'undistorted life' would have done so for Robson. Within a tradition which normally would accuse bourgeois ideology of drawing dualistic polarities, an apparently sharp disjunction between pre-capitalist 'nature' and capitalist 'civilization' remains. And it is used to highlight the ills of capitalism by putting them against an imaginary setting, almost as if history began with industrial capitalism. The setting is essentially empty and static, and exists only as the 'other' which capitalism has wrenched away. In that sense, it is no different from the bourgeois literature on the family, which posits natural communities and extended families as a kind of prehistoric, natural world before the time when capitalism fragmented

them into isolated units of socialization, elastic labour supply and temporary escape (Marxist critiques by Eley, Vogel).

Robson blames the hospital for the kind of medicine we have now. Given the common thrust of the various critiques of medicine, many of which agree in attacking the eclipse of preventive medicine by hospital medicine, we can understand the desire to discover the modern distortions of health care in the emergence of the hospital as a medical centre. The modern form of disease-centred, acute medicine had its material roots and expression in the hospital movement common to western countries in the late 18th and 19th centuries. The objectification and expropriation of illness as disease, the contact between elite doctors and poor patients (a confrontation between social classes) and the emergence of medicine as a sector of capital accumulation are rooted in the history of the hospital. So, at one level, Robson is right to make this connection. But the hospital itself must be seen as a mediator, a focus of forces, rather than an autonomous institution which has driven medicine along its modern path (Rothman; Scull, 1977; Katz; Foucault, 1973, pp.84-5).

Hospitals were only one form of institutional growth from the 18th century onwards. In Britain, they were founded at first with money from subscription at a local level. They had little to do with technology or capital-intensive procedures. That is, they did not reflect a new scientific medicine; they embodied new social relations, and, in the process, they also opened a space in which disease-centred medicine developed in line with scientific changes at large — themselves also socially rooted.

Looking at the hospital as one of a set of material foci of social relations means we have to understand their historical specificity, not just their impact on medical practice. For example, in Britain they were sometimes intended to ameliorate class antagonism, in that the subscriptions were solicited from all classes to support a common project. Their history is partly the history of charity; but it is also the history of the mobilization of labour through separating unproductive people from productive ones, and the history of working class discipline, through encouraging saving against possible incapacity. Their history is certainly that of hegemonic class relations and supervision as well, in that the bourgeoisie brought the ill into its own institutions, and enforced bourgeois habits through its medical regime (Manchester, Rosenberg).

I don't want to belabour the history of medicine, except to make the point that we must look at moments in this history, such as the emergence of hospital medicine, as mediations of social relations. They share in a momentum; they don't initiate it themselves. Robson takes the French Revolution to exemplify a struggle to reassert an undistorted health policy, and the Parisian hospitals to embody the distortions of hospital

medicine. The striking feature of these hospitals was their enormous size, the class relations (professional elites and poor patients in a public facility) and the clinical practice (the demonstration of *discrete* diseases, both during life and after death). One could see in the scientific aspect of this hospital medicine a mediation of the social relations crystallized in that setting. The science (nosology, pathological anatomy, diagnosis of discrete diseases) *was* the social relations; the hospital *was* the location in which they were enacted. More on this later (Figlio, 1977; Foucault, 1973; Jewson; Waddington).

Britain was different. Here, as I mentioned earlier, the teaching hospitals were private charities or community projects. The equivalent institutions were the poor law infirmaries, whose combined bed capacity far exceeded that of the voluntary hospitals by the late 19th century (O'Neill, Webb). These hospitals embodied the social relations of class quite explicitly. The poor law physician decided on the applicants' regime and fitness for the work house to which the infirmaries were attached; the work house under the Poor Law Reform of 1834 acted to mobilize labour by deterring the able-bodied from seeking relief and by taking in the genuinely destitute, dependent, or incapable (Checkland, Scull). What happened to the applicant, the regime and the diet depended on the doctor, a paid public officer and a professional. Indeed, the Poor Law service helped to establish medicine as a new professional career for a large number of people (Crowther, Inkster, Peterson).

These infirmaries become Britain's general hospitals (Ayers). But here, as in the case of the Paris general hospitals, what stands out is not the practice of a proto-scientific medicine which distorted the very notions and practices around health and disease; what stands out is the sharpened edge of social relations brought to bear in an institution — indeed, in a building.

I don't think Robson assumes that hospitals did emerge in the wake of a new medicine; his argument builds from a commodity mentality in medicine, not only from the institution itself. Yet he does say:

'I feel that, in the past, one's main conception of health was defined and appropriated from a very strange institution — the hospital — and this led to a very mechanistic conception of health ('Corporate . . .', p. 24).'  
'... up until recently the hospital has been the main institution for definition of disease . . . (p. 22).'

I emphasize the role of the hospital in his argument because, like 'stress' and 'community' in the case of Eyer and Sterling, it is potentially both utopian and co-optable. It is utopian in the same way that personifying the effects of capitalism is — by riding on an implicit dualism between a 'natural' state before the person/institution distorted it, and a 'social',

i.e., changeable, state imposed by them. One can see the same orientation in the polarity between acute = hospital = professional = hierarchical = elite medicine, and preventive = primary = de-professionalized = democratic = people's medicine. 'Democratic vs. professional' carries the programmatic political message:

'Deprofessionalization of health care, and the bringing of health care activity and health workers' education under the democratic control of all health cadres and the general population.'

'The distribution of health care resources in the mass social interests to be ensured by the democratic control of allocation at all levels (Segal, par. 11).'

There's nothing in the middle, standing between capitalism and the sick person. When critics attack the hospital and its disease-centred, acute medicine, we expect a textured understanding of these mediations to emerge. But the hospital shows up either as an institution to be assaulted in the re-conquest of undistorted health and illness, or as just a translucent trace of capitalism. If it is granted enough contradictory, human substance of its own, it is as the location of labour unrest, not as the site of redefining health and health policy. As Julian Hart has rightly argued, the nihilistic attack on the hospital without clear alternative strategies combines effortlessly with conservative economic policy to cut public spending (Hart, 1976, p. 19; 1977, pp. 10-11).

I've gone through the implications of three quotations, taking the liberty to range somewhat freely around them. I don't want my remarks to be taken as an attempt to recount the detailed positions of these authors. They stand for three styles of argument. I started with the dualism of 'nature' vs. 'civilization', where it was easiest to see complete absorption within capitalist ideology. In other words, we're up against nature, and science will change it. I went on to a sympathetic position which articulated a gut reaction to the submergence of human needs beneath those of capitalism, and also the craving for a simpler communitarian way of life. It did open up a mediating zone — a place to detect the pathogenicity of capitalism and resist it — but it also retained a 'nature'/'civilization' dualism in the muted form of community vs. capitalism. Finally, I went from this radical formulation to the more politicized left critique, only to discover a similar dualism in the language of 'undistorted' vs. 'distorted'. Somehow the specifically medical domain vanishes at this point, to be sorted out in the eventual elimination of medicine as a commodity. What runs through all these positions, from liberal scientific to radical left, is a form of the usual bourgeois dichotomy between nature and civilization (read 'capitalism'). What shows up in relief with the dualism is a vacancy, a silence about the mediations which make medicine special *and* capitalist, and which offer possibilities for conceptual and political practice.

### Illich and Navarro: Mediations and Revolutions

I'm not going to review Illich and Navarro; that has been done often enough already (Horrobin; Partridge; Robson, 1976; Tudge; Carstairs, 1974; Watts; Hart, 1978; Berliner; Maynard; Ferguson; Bury; Gil; Carstairs, 1977; Klein). I want to use them as critical stances sufficiently well known to provide tools for opening a further discussion. I want to be free of the stereotypes, especially of Illich, which only force us to cover heavily trodden ground again.

Illich and Navarro represent two poles — perhaps two different registers — of the critique of modern medicine. For Illich, medicine shares the suicidal course of modern society in its unlimited reliance on technology; for Navarro, medicine shares social and physical inequalities of modern society in its replication of socio-economic hierarchy. For neither critic is medicine an autonomous area of domination or of struggle for liberation. But their similarities seem to end there. Perhaps the simplest way to bring their disagreement into focus would be to quote from Illich's response to Navarro's criticism of the preliminary study for his *Limits to Medicine*:

'Navarro argues that the prevailing values in the health sector are indeed shaped by the health establishment, but are symptomatic of the distribution of economic and political power within society. The power to shape health values gives the professionals within the health sector a dominant influence on the structure of the health services, but actually no control. This control is exercised through the ownership of the means of production, reproduction, and legitimation held by the capitalist elite. Navarro does not seem to realize that I do agree with him on this point but am less naively optimistic as to the political indifference of each and every *technique* used in the provision of health care. I argue that dialysis, transplants, and intensive care for most chronic diseases, but also just the general intensity of our medical endeavor, inevitably impose exploitation on any society that wants to use them in the repertoire of its medical-care system (Illich, 1976, p. 251, n. 83).'

Illich opposes the dependency on a bureaucracy infused with technology, which undermines self-reliant, autonomous coping and healing. Navarro responds to Illich's assertion:

'Actually, I consider those bureaucracies, be they trade, services, or "whatever", to be the mere socialization instruments of those needs, i.e. they reinforce and capitalize on what is *already* there — the need for consumption, consumption that reflects a dependency of the individual on something that can be bought, either a pill, a drug, a prescription, a car, or the "prepackaged moon". Indeed, the overall quantum of citizens' dependency is far more than the mere aggregate of dependencies of those citizens on the bureaucracies of our societies. Actually, those dependencies are mere symptoms of a more profound dependency that has been created in our citizenry not by industrialization, but by the capitalist mode of production and consumption... (Navarro, 1976, p. 112).'

In Navarro's eyes, Illich locates the failure of medicine in industrialism and a derivative social formation, the bureaucracy that administers it. That makes him, along with Galbraith, a proponent of the 'convergence thesis', that different political systems converge around the needs of

industrial organization. Navarro resists any perception of medicine which blunts political engagement. Industrialism and bureaucracy are, for him, only middle terms, fetishized by Illich. Power remains in the hands of the powerful, not in the hands of their servants — including doctors. Change cannot come about by withdrawing from the brink, as it were, and reforming self-sufficient communities which simply refuse technology. Illich gives him an easy target with his apparently simple rendering of the seeds of change:

'In several nations the public is now ready for a review of its health-care system. Although there is a serious danger that the forthcoming debate will reinforce the present frustrating medicalization of life, the debate could still become fruitful if attention were focused on medical nemesis, if the recovery of personal responsibility for health care were made the central issue, and if limitations on professional monopolies were made the major goal of legislation. Instead of limiting the resources of doctors and of the institutions that employ them, such legislation would tax medical technology and professional activity until those means that can be handled by laymen were truly available to anyone wanting access to them. Instead of multiplying the specialists who can grant any one of a variety of sick-roles to people made ill by their work and their life, the new legislation would guarantee the right of people to drop out and to organize for a less destructive way of life in which they have more control of their environment. Instead of restricting access to addictive, dangerous, or useless drugs and procedures, such legislation would shift the full burden of their responsible use onto the sick person and his next of kin. Instead of submitting the physical and mental integrity of citizens to more and more wardens, such legislation would recognize each man's right to define his own health ... (Illich, 1976, pp. 272-3).'

Between Illich and Navarro, we really seem to be trapped with alternatives which speak neither to the dramatically urgent medical emergency which has become the model for our consciousness of health needs, nor to the possibilities for change. For Illich, technology is autonomous, and brings both its own rewards and punishments; for Navarro, technology is transparent to the class structure of society. Illich's analysis requires radical transformation of society by individual alternative behaviour in the face of industrial society; Navarro has so completely subordinated medicine to the structure of capitalist society that any change requires the end of capitalism. For Illich, technology is self-alienation — it is the loss of autonomy and integrity. For Navarro, technology is a mediation of capitalism as it now stands, but its transparency precludes any analysis of mediation. It represents capitalism to the individual, but it is not a potential arena of struggle because health is not an arena of struggle. That's oversimplified, but I want to avoid putting in 'autonomous' in describing health as an area of struggle. That would lead to endless debate over 'relative autonomy' without altering the fundamental opposition between Illich and Navarro.

There is a sense, however, in which their positions are quite similar. Both Illich and Navarro presume a state compared with which the present is a distortion, and that presumption robs health and sickness of a meshing with day-to-day experience which might have potential for political

awareness and change. Each of them can attack the other for a failure to bring radicalism to the point of revolution. (Illich is too voluntaristically naive; Navarro is too determinist.) Both are sensitive to how radical sounding reform can decay into reaction without a standard by which an irreducible novelty can be sustained.

Their disagreement brings me to a crucial dilemma. Profound as their differences may be, they are not the source of trouble for medicine on the left. Illich's analysis can be picked apart, but putting 'capitalism' in place of 'technology' will not help very much. I suspect that there is more a dissonance because of there being two different registers next to each other, than there is the possibility of a rebuttal of one position by the other. Dissonance lacks the very shared terms of reference which a rebuttal would demand.

Let me develop this point by expanding on Illich. Illich looks at several varieties of medical criticism: consumer protection, equal access to medical services, public control of professions, the conflict between scientific and healing orientations, environmentalism. Each of these potentially radical positions, he argues, contains the seeds of its own decay because it does not grasp the extent to which the failure of health resides in technology. Thus, to pursue their goals on their own is to aggravate the overall sickness.

Illich is ambiguous about the relationship between technology and sickness. I take him to mean, not just that machinery tends to create dependence, but that its very existence *is* the dependence. That's why I said that technology *is* alienation. It requires gross inequality to bring it beyond a certain level of development, and to do that also requires a loss of personal and social integrity which, in turn, the presence of the machinery materializes, stabilizes and exacerbates. The process is dialectical, in that subject and object — person and nature — are formed by it. And neither social relations, nor self-awareness, nor technology can change apart from each other.

Modern medicine, embodied both in the doctor and in technology, struggles against death. Illich locates the emergence of the notion of a natural death at the close of an unperturbed life with the rise of the bourgeoisie. The idea of life as a capital investment amortized only at the end of a natural life span, like that of well attended machinery, comes from the same calculating class. Life insurance and expectancy tables date from the flowering of this class in 17th century England. 'With the rise of the bourgeois family, equality in death came to an end: those who could afford it began to pay to keep death away (Illich, pp.189-90).' Finally, doctors would determine when death would be permitted. 'It is

the point at which a consumer, trained at great expense, must finally be written off as a total loss. Dying has become the ultimate form of consumer resistance (pp.206-7).'

The backdrop for the heroic battle that Brian Hurwitz talks about was set in this context. It is a battle that, in many circumstances, we applaud. It is only when we discover to what extent it has been bought for a few at the cost of the many, and when its failure often to deliver the goods on its own terms suggests an omnipotent denial of death rather than the provision of health, that we ask what social relations are crystallized in this apparently autonomous institution.

Illich does not, in the way Navarro claims, dislocate these changes from class structure. Such a reading is only possible with a reductive notion of technology as the things doctors use, rather being itself a form of 'frozen' social relations (R. Young, 1977). But he does take technology to be a universal category and, in the process, muddies the water as much he clarifies it. It is a very simple, but very urgent, fact that technology for us *is* capitalist. It is pure abstraction to think of our technology in any other way. That means that the particular forms of inequality, of class, of self-awareness, of political, economic and social relations, and of technology, are also forms of capitalism. Illich correctly materializes alienation in technology, but then releases it as if it were an autonomous phenomenon. That simply won't do. And the consequence won't do either. We can no sooner reduce our level of technology (taken in both the broad and narrow senses) than we can voluntaristically reduce our commitment to capitalism. I don't know if this is a universal law, but it is the concrete historical condition of our technology.

Illich is caught in a reification of technology. The mad dream of a 'plastic womb' (p.257) is not just a dream of an anesthetic life; it is an omnipotent fantasy of immortality. But to accept mortality does not require us to turn away from the relief of toil, suffering and 'unnatural' death. The goals of extending life and of reducing suffering remain the same goal until the former actually compromises the latter, either in one suffering individual or in others who are sacrificed by inequality to an abstract aim of longer life. There is no solution to the dilemma. Extending life is often the same as reducing suffering, either by actually stopping a pathological process or by relieving unbearable pain or by extending the period for coping with dying. To imagine that the resolution of the meanings and relationships among dependency, suffering, coping, dying, inequality can derive from the limitation of technology is to isolate technology from the historical processes which constitute it. Illich seems to think that reducing technology will control the omnipotent fantasy, which, in turn, will reduce the dependency which needed technology in the first place. But that's an abstract cycle, spinning on its own imaginary axis. It derives technology

from fantasy, and it therefore assumes that limiting technology and fantasy is a political act which can be sustained outside (or beyond) engaging with the system that drives the technology, the inequality, the overall increase in suffering, *and* the fantasy.

Because it is a great refusal (Marcuse) of technology and the bureaucracies that administer it, Illich's position has no politics but refusal. There is no process for redefining and reclaiming health, no connection with the recognition of oppression in other areas, no integration of the struggle for health with the struggle for liberation from exploitation. There is no action on the ground, other than legislation.

Because there is no politics, or, rather, because the politics is collapsed into the technology, Illich does appear to be cruel. Not only does he not open paths for collective change, but he necessarily misconstrues the technology as it now is. Remember his rebuke to Navarro, that he did not understand the political meaning of 'each and every technique', including dialysis, transplants, and intensive care, as well as the intensity of the medical endeavour. With no prior political structure to stabilize, the technology must itself become exploitative. But why can't it equally relieve, sometimes 'cure' (in the sense of eliminating death as an immediate threat) and certainly help with coping? Uta Gerhardt's forthcoming study of renal dialysis points precisely in this direction. Also, with no embeddedness in the social relations that form it, technology is not part of social change. Illich offers no alternative to rudely separating those who suffer from those who do not, because he does not extend the range of collective action to include both freedom from oppression in everyday life, and freedom from unbearable suffering; coping applies to everybody. But learning to live with limitation and mortality have to become more than a refusal of advanced medicine. Otherwise, those who face them are isolated from those who are actively engaged in creative collective change, or everybody sinks into the same politics of negativism.

Navarro rejects both Illich's technological determinism and his separation of consumption from production, which transforms the oppressiveness of capital's control of work into a problem of consumerism. It does surprise me that Illich, who castigates the various consumer approaches to medical reform so strongly, should himself rely on a grand consumer withdrawal. Navarro tells a more convincing story from the standpoint of political priorities and relationships of power. Illich speaks more to the levels in between the structure of capital and the formation of human consciousness. Somehow, the two levels have to come together.

## Medicine as a Commodity

Neither Navarro nor Illich paid much attention to the ways in which objects mediate social relations. Navarro subordinated technology so completely to political structure, that it had no density of its own. Illich dwelled within it to the exclusion of seeing what technology contained of those structures in its very make-up. Defining this intermediate area, in which objects command us because we built the commands into them, in which objects are revealed as 'frozen' social relations, is precisely what Marx began with his exploration of 'the fetishism of commodities and the secret thereof' (Capital I. ch.1. sect.4). In the inversions of fetishism, the mutable relations between people are represented as immutable relations between things. Bob Young (1977) has expanded that analysis to include all the objects of human execution, including scientific theories. Les Levidow has used the same approach to mount a marxist critique of left positions in the I.Q. debate (1978). Critics of medicine on the left have sensed the importance of this metaphor (perhaps I should say 'resolution' because the purpose of the metaphor is to support the resolution of objects into the social relations bound into them) for understanding the characteristics of modern medicine. I want to scan some of these ideas now, then try to elicit the meanings implicit in them.

As a way into it, let me first just give a selection of the uses of 'commodity' in health care:

'The transformation of health care into a commodity, and the production of surplus value in the health care industry, are central features of contemporary health care organization . . . An intrinsically social, cooperative process of healing has been transformed, under capitalist medicine, into a commodity exchange process . . . this intrinsic part of human social life has become a capitalist commodity . . . (Rodberg and Stevenson, pp.104, 114).'

'Renaud has demonstrated how capitalist growth, while giving rise to disease patterns in society, also institutionalizes "solutions" to disease which are compatible with capitalism, in the form of a commodification of health services (Salmon, p.126).'

'When health care is a commodity, a fundamental contradiction arises between its quality as an exchange-value and its quality as a social use-value (Segal, sect.1).'

'Conflicts will remain so long as health is considered a commodity and the patient a vehicle which brings disease to the doctor (Rossdale, 1966, p.9).'

'However, before the health service could be influenced to meet [the objectives of a healthy population], the whole concept of health must be examined. Robson asks "in what way has capitalism contributed to our present concepts of health?" and gives the answer that "the root of the ideas . . . is that of disease as a commodity" (Mullen, p.12).'

'Medicine as a commodity is not necessarily concerned with health (Radical Statistics, 1977, p.6).'

'Commodity' is a rich term, especially when it is analyzed as a fetish which hides social relations behind a facade of objective reality. It's richly ambiguous in another way too; it suggests, not only the sharing of one particular commoditized process in a general commodity ideology, but

also the existence of a capitalist labour process in that particular sector. That ambiguity with respect to medicine as a commodity invites us to look both for capitalist ideology in the concepts of medicine and for capitalism in the practice of medicine.

The ambiguity is suggestive, but it is also confusing; I've spent a lot of time trying to understand just what it means to say that medicine is a commodity, without knowing how it is useful beyond the first jarring impact. That impact, however, is real and important. I was talking about this paper with a hospital doctor and reporting on the debate about medicine as a commodity. He reacted, as it happens in the comic strips, as if someone had switched on one of those light bulbs of sudden recognition — even though neither of us could keep the bulb burning very brightly. I think that is because the commoditization of medicine is more a metaphor than a shorthand description of medicine, and because it is too general; it becomes, when you push on it, a definition of capitalism in general, so that medicine as a commodity *is* capitalist medicine and is coterminous with capitalism.

### **The Medical Commodity and the Metaphysics of Medicine**

Thus to say that medicine under capitalism is poisoned by the separation of exchange-value from use-value (Segal) is to describe the contours of everything that happens under capitalism, even if no precise formulation of value-creation is possible. Value is at the heart of the problem, but more as an historically moulded matrix of social relations, than as a powerful determinant of medical practice. David Dickson's article (*RSJ8*) is very good on this point. He speaks of the formal consequences for science of the emerging capitalist labour process. The labour process during the Renaissance and the 17th century replicated in its social relations certain formal elements of knowledge, which then became the metaphysics of modern science. And that metaphysics then set the terms and the procedures by which science would proceed as a self-consciously new enterprise with people like Bacon, Descartes and Newton. The Down survey, which made William Petty a rich man, enacted in a social and a conceptual register what was also an economic process. Petty paid himself by the area surveyed, and paid his men by the distance surveyed — a kind of built-in surplus value. His plans required only menial skills from his men, and the instruments he built incorporated his own expertise into the procedures. In that labour process, in addition to automatic surplus value, Petty inserted a sharp separation of manual and mental labour, leaving a deskilled manual labour process, a stratified labour force, a clear methodology and scientific instrumentation which generated new data. Behind it, conditioned by it even if not an explicit part of it, lay the metaphysical distinction of primary and secondary qualities

(i.e. matter and motion vs. conscious imputation of meaning) which underlay modern scientific thought. The new *form* of the labour process constituted the basic metaphysical framework of a new mechanistic science.

As a background, David Dickson constructs a picture of the meta-physics of modern science from the emerging stratification of the labour process of artisanal work in the Middle Ages. Some craftsmen became the capitalists who operated between wealth and workers to whom they sub-contracted. They became the planners of others' labour. The unified craft tradition of plan and execution, of thinking and working, split into planning and contracting vs. selling labour. That kind of stratification of the labour process lay at the heart of the emergence of capitalism and capitalist metaphysics; the social relations enacted the ideas.

The social construction of metaphysics is a multi-layered phenomenon. David is talking about a restructuring of the formal features of the labour process which began in the 14th century and ripened in the 17th. During the same period, world trade developed along with systematic accounting of the flow of money and of national wealth. That growing sense of economic activity, determined by impersonal laws of the circulation of money, peaked in the 17th century. Pamphlet wars raged around the relationship between the value of precious metals and the value they represented as money. In England, that dispute also embodied a political struggle of the bourgeoisie over arbitrary monarchical power — waged as a battle of natural lawfulness vs. human whim, bourgeoisie vs. aristocracy. On the surface, however, it was a dispute about how to remint coins which had been clipped to get more precious metal value for a given face value (Appleby). The economic, political and social relations carried along with them, as in David's example, the new metaphysics of determinate lawfulness which conferred power through knowing the laws.

The market place became the social process which enacted lawfulness — the subjugation of the individual to these determinate laws, of quality to quantity. Hobbes spoke of honour as the value of a man, and value as what was granted by the esteem of others which, in the last instance, meant what could be commanded (*Leviathan*, ch.10). Relative values of people were relative abilities to mobilize others in a free-trade economy of powers — what, literally, could be bought. The market place re-constituted the moral virtue, honour, into a measurable and comparable quantity. Not too far in the background was the split between primary and secondary qualities, between the determinate motion of bodies — the lawfulness of the physical system — and the effect of physical motion on consciousness.

Within this form of social relations, a relationship became a transaction,

and a transaction became an exchange which, by definition, exchanged equal values. Although, from the standpoint of accumulation, medicine remained on the margin of capitalist interest until the late 19th century, it still concretely shared in capitalist social relations and ideology, just as did the natural sciences. Take the revolutionary assertion that the blood circulates. Marx ridiculed any direct homology between economic and bio-medical laws, i.e., that the circulation of money was like the circulation of the blood. But there were striking formal similarities between political economy and physiology, even if not at the level of obvious homology. The circulation of the blood, clearly enunciated by William Harvey in the mid-17th century, cut across the hierarchical relations of direct dependence between organ systems which characterized physiology before then. Indeed, the reaction to the theory of circulation was couched in terms of its destroying the very basis for pathology and therapeutics which lay in these hierarchical relations between abdomen, chest and head — liver, heart and brain (Mani). Circulation of blood disregarded the distinct properties of these organs, and demanded a new form of transaction between them. Similarly, Marx described the replacement of direct social dependencies of blood, position, education, by the mediated relations of the money economy. Beneath the apparent freedom of individual collisions in the market place:

'These external relations are very far from being an abolition of "relations of dependence"; they are rather the dissolution of these relations into a general form; they are merely the elaboration and emergence of the general *foundation* of the relations of personal dependence (*Grundrisse*, p. 164).'

Likewise, the circulation of the blood mediated the exchange between separate producers in the body and the growth of each of them. This was quite different from the previous relationship, in which the separate regions interpenetrated each other, each supplying a unique vehicle for conveying its own influence. Now, through a common medium of exchange (a universal equivalent), each producer participated both in common growth and in its own separate growth. The *act* of circulation, not the qualities of the various sectors, achieved this enlargement and maintenance. Marx granted a similarity at this level between the political and the physiological economy:

'The circulation of capital is at the same time its becoming, its growth, its vital process. If anything needed to be compared with the circulation of the blood, it was not the formal circulation of money, but the content-filled circulation of capital (*Grundrisse*, p. 517).'

Qualitative physiological differences, as with values in a moral and social sense, were now mediated through a common, law-like process. A set of social relations enacted a metaphysics. The metaphysics laid down a matrix within which the perception of natural phenomena and the methodology for studying them were formed. Medicine participated in this, not

by an abstract logic, but by the concrete evolution of medicine within the social relations of capitalism.

The models of the body in health and illness also shared directly in the metaphysics, the methodology and the laws of the emerging natural sciences. René Descartes, Marcello Malpighi, Hermann Boerhaave, Albrecht von Haller, Niels Stensen, Walter Charlton, Richard Lower — the list could go on — all attempted to include the living body in a mechanical account of movements. As much as they tried to show the action of physical laws behind distinct vital processes, such as glandular secretion, they also put forward a methodological approach to grapple with living phenomena. That is, they saw the 'new' science as a way of proceeding, not just as a set of discoveries. The 'machine analogy', so often associated with Descartes, was precisely such a methodological plan. While he spoke of levers and tubes as models for anatomical parts, he primarily intended to use them to illustrate the processes to which mechanical and mathematical reasoning applied and to equate these processes with the knowable. They introduced a relationship between secondary qualities (consciousness) and primary qualities (matter in motion) and a reduction of all primary qualities to commensurability — i.e., to measurement within a common framework (as honour = exchange-value). To look for the tiny machines — the tiny machine-like processes — became a metaphysics of living organisms and a methodology for studying them:

'The machines of our body, which are the basis of medicine, are made up of cords, filaments, beams, levers, tissues (tele), fluids coursing here and there, cisterns, canals, filters, sieves, and similar mechanisms. Man, examining these parts by means of dissection, philosophy, and mechanics, has learned their structure and use; and, proceeding a priori, he has succeeded in forming models of them, by means of which he demonstrates the causes of these effects and gives the reason for them a priori. From a series of these, aided by ratiocination, and understanding the way Nature operates, he founds physiology, pathology, and then the art of medicine (Malpighi, vol. 1, p. 570).'

In this tradition, microscopic anatomy and gross anatomy flourished. Indeed, they were co-extensive with each other. The microscope was as much the material form of the new metaphysics of the living body as it was an instrument for seeing fine structure (at which it wasn't very successful). The practices of anatomy and physiology were informed by this metaphysics, and they included pathological structures and functions too. Thus, the new 'physical' medicine emerged within a metaphysics which reduced the qualitative distinctiveness of structures and functions, in health and illness, to commensurable processes — to a small set of phenomena to which a common measure could be applied.

Why go through all this? I want to show that the way medicine looks at illness under capitalism does not bear the stamp of capitalism always on

the surface. Social relations which may have formed with the changes in the labour process described by David Dickson carry ideological consequences as part of their history. That history informs areas apparently far from the crucial changes in the labour process, but only through layers of mediations. I think it is reasonable to say that ways of seeing the living body, such as microscopic anatomy and circulatory physiology embody, were capitalist, in the sense that they presupposed metaphysical re-orientations (Quality reduces to commensurable quantity; the process of circulation implies growth; disease is malfunction — another reduction of quality to quantity) which the changing social relations of production first enacted. The 'polluting' exchange-value of the commodity, health care, is more than a feature of the medical transaction; it is built into the metaphysical framework within which living processes are perceived. Tending to take for granted that framework, we are left today with only the residue which we call objectivity. The framework itself, the social relations which informed it, were articulated in the period David Dickson talks about.

### **The Medical Commodity and Capital Accumulation**

I've been talking about the way the social relations around exchange value have been built into the conceptual structure of medicine. Hopefully, in this way we can avoid any overly easy identification of particular features of modern medicine with an essentially capitalist core. For example, whenever we refer to practices such as 'fee for services', we are likely to get confused over whether or not that practice embodies the essentially capitalist nature of medicine. Let me just briefly look at fee for service, then move on to the other implications of commoditization of medicine.

Fee for service can mean precisely what the words say, a payment for a personal service. As such, it lies wholly outside the capitalist system. Of course, any service *might* contribute indirectly to accumulation, perhaps by serving a capitalist in some way essential to his/her function as a capitalist. Figuring out whether or not that is so can become an endless project in drawing ever finer logical distinctions. Marx became rude at the prospect:

'Labour as mere performance of services for the satisfaction of immediate needs has nothing whatever to do with capital, since that is not capital's concern . . . A. Smith was *essentially* correct with his *productive* and *unproductive* labour, correct from the standpoint of bourgeois economy. What the other economists advance against it is either horse-piss . . . , namely that every action after all acts upon something, thus confusion of the product in its natural and in its economic sense; so that the pickpocket becomes a productive worker too, since he indirectly produces books on criminal law . . . Or the modern economists have turned themselves into such sycophants of the bourgeois that they want to demonstrate to

the latter that it is productive labour when somebody picks the lice out of his hair, or strokes his tail, because for example the latter activity will make his fat head — blockhead — clearer the next day in the office . . . All this is nonsense. Digression. But return in more detail to the productive and unproductive (Marx, *Grundrisse*, pp. 272-3).'

We mustn't draw conclusions one way or the other about the capitalist nature of medicine — or the absence of it — from the presence or absence of fee for service. Fee for service is not co-extensive with capitalism; nor is contract service co-extensive with the National Health Service. In fact, the option of a career in medicine in Britain is largely a 19th century phenomenon built on public or salaried service (e.g., the Poor Law or contract practice with sick-clubs, to which one could add the establishment of posts for factory surgeons and Medical Officers of Health). I know of no estimate of the total medical staff in all these institutions. By the late 19th century, however, the bed capacity of the public hospitals which developed under the Poor Law far exceeded that of the private voluntary hospitals (Webb, p. 151). Poor Law doctors in the infirmaries of England and Wales numbered 4841 in 1914 (Crowther) and there were 3713 District Medical Officers in 1909 (Webb, p. 145, n. 3).

There is something deeper than fee for service implied in the selection of quotations about medicine as a commodity. It has to do with the ways medicine either is a sector of capitalism, or appears to be one, or interacts with capitalism. To say that medicine is a commodity implies that it grows from productive labour in the same way as other commodities do, and that medical commodity production is as indifferent to the nature of the commodity as other commodity production is. It is to say that, no matter what the sector, commodity production is dedicated to the accumulation of capital. Use-values may be necessary at some basic level, but the motor of commodity production is accumulation. Seen as the location of accumulation, medicine as either productive or unproductive labour becomes an important issue because it forces us to look at the social relations rather than at the product:

'Capitalist production is not merely the production of commodities, it is essentially the production of surplus-value . . . If we may take an example from outside the sphere of production of material objects, a schoolmaster is a productive labourer, when, in addition to belabouring the heads of his scholars, he works like a horse to enrich the school proprietor. That the latter has laid out his capital in a teaching factory, instead of in a sausage factory, does not alter the relation. Hence the notion of a productive labourer implies not merely a relation between work and useful effect, between labourer and product of labour, but also a specific, social relation of production, a relation that has sprung up historically and stamps the labourer as the direct means of creating surplus-value (Marx, *Capital* I, ch.16, p.477).'

Are the social relations of medicine capitalist social relations? The answer has more than one part. Marx seemed to say that capitalism existed wherever a capitalist accumulated capital by extracting surplus value from a worker, no matter what s/he produced. Elsewhere, he

returned to the question of capitalist services and tried to discriminate their capitalist content in terms of their service to the accumulation of capital from commodity production. Railways, for example, became productive capitalist services only by adding to the value of commodities through productive labour in bringing them to market. That people, even productive workers, also used the railways was irrelevant — that satisfied a want, but added nothing to value. Transport necessary for consumption of commodities, either during or after production, became part of their use-value; transport in which capital accumulation occurred added to exchange-value; transport of people only satisfied their wants, i.e., it was valueless (*Capital* II, ch.6.iii, p.153; Mandel, pp.405-6).

According to Mandel, the distinction between production and circulation was fundamental in Marx's thought. Even services directly involved in capitalist operations, such as banking, contributed to the latter, not to the former. And if that separation between production and service held for banking, then it held even more clearly for services, such as health care, which lay further down the line from production (Mandel, pp.404-5). *Individual* capitalists, Mandel argues, may still appropriate surplus value from their own workers who produce a service. The question for Mandel is not whether appropriation occurs, but whether accumulation occurs for capital in general. An individual capitalist might either add to that general social product in the process of his/her local accumulation, or s/he might feed on it by appropriation. From the individual standpoint, both processes accumulate capital; from the standpoint of capital in general, only the former does.

Why bother? Well, the distinction throws some light on the capitalist nature of medicine, which might elude us if we accepted too readily the obvious differences between fee-for-service medicine and National Health Service medicine. It also makes sense of the 'crisis', which affects American medicine as well as British medicine. Let's look at the similarities of medicine within capitalism.

At one level, the American medical establishment is another world from the British. Most doctors practice fee-for-service medicine and work in private hospitals. Even charitable, non-profit hospitals accumulate capital. Their problem becomes one of rapid expansion and competitive buying of new equipment to attract staff, because their average return of about 3% on total revenues cannot be paid as dividends (Rodberg and Stevenson, p.109). Nonetheless, the return is there, and the competitive advantage of a hospital depends on it. Furthermore, non-profit medical care is financed through insurance companies. Even the proposed national schemes before Congress are, with one exception, schemes for national insurance, not for a national health service. Just as with the more limited national schemes for the young, the old and the poor today, they

are to be financed through insurance companies, not through general tax revenues.

Some of the more glaring atrocities of American medicine seem to follow from this fee-for-service organization and the implicit profit-making of the non-profit sector. The orientation of health care towards high cost technological medicine not only subordinates primary care, it also leads to wasteful duplication of facilities whose insufficient use means less practised medical care. It may also lead to unnecessary medical interventions, because the climate encourages more medical procedures (Radical Statistics, 1977). It may also lead to doctors whose wealth, pretensions and backgrounds separates them increasingly from their patients (Ehrenreich, in Ehrenreich (ed.)).

But my recent experiences of both systems (US and UK) give me no fundamental sense of their differences. I have lived close to 'centres of excellence' in Britain, i.e., area health authorities built around teaching hospitals and, during a recent visit to the US as a lecturer, I also lived close to a 'centre of excellence' at a University. In both cases, the facilities have, literally, been exemplary and probably pretty similar. In both cases, I have been financed — by an employer-negotiated group insurance plan in the US, which cost me only about 1.3% of my income, and by the NHS in Britain, which cost me an unknown, but unfelt, amount. In other words, I have been sheltered from the economic discrimination of American medicine and from the regional discrimination of Britain. But in America I had to find a doctor willing to sell his/her services; also, not all services are covered by insurance, and there is always the threat of a medical catastrophe, in which costs will outrun insurance plans. People are frightened of going bankrupt because of illness, and insurance companies manipulate this fear by drawing up interlocking policies — different potential medical scenarios are separate exploitable markets.

Nonetheless, my experience, and that of most of the people I know, does not really discriminate between the two systems. Social class, income, geographical location, race and sex affect the incidence of illness and the access to medical care in both countries. The style of medicine practised is the same. The differences must not obscure the similarities.

Taking Marx's and Mandel's analyses of the relationship between services and productive capital, I think the significant economic difference between British and American medicine is not public vs. private, but service vs. commodity production. That is why there are similar demands from the state and capital in general to restrict expenditure on health care. It is counterproductive to rest a case for the NHS on differences between British and American medicine before sorting out the fundamental similarities — the NHS has not introduced a different form of

medicine into capitalist society. To try to argue that it has can lead only to endless tit-for-tat scoring.

Just as it would be a mistake to assume that we have to justify the NHS on grounds of its medicine, it would also be misguided to assume that we have dealt a blow to capitalism by attacking the technological medicine which capitalism has developed. Capital stands ready — more avidly ready than the NHS — to move into any sector where accumulation may occur. If such a sector is one which, like preventative medicine, seems to represent a different style of medicine from capital's usual medicine, then uncritical support for it can also become unreflective denunciation of the NHS. Preventative medicine is big business in America, and it could be big business in Britain (Berliner, p.120; Hart, 1977, p.10). Preventative medicine might be good to emphasise within a strategy for better over-all health care, but it must be won within the socialist foothold opened by the NHS. British United Provident Association (BUPA), for example, is keen, not to fight over private medicine within the NHS, but to open capitalist sectors outside the health service. In this respect, the pay beds (i.e., private beds within NHS hospitals) are a red herring. Indeed, the Conservative Party worries BUPA more than the Labour or the unions do because the Conservatives might obfuscate the issue of capitalist gains by an ideology of private enterprise. Pay beds within the NHS are ultimately in conflict with the growth of the private sector (Phillips). BUPA wants the phased withdrawal of private beds to continue:

'[The Health Services Act of 1976] has formalized the existence and *raison d'être* of the independent health sector and far from weakening its position the legislation has proved instrumental in bringing about the much needed stability, whereby consultants use independent hospital facilities, occupancy levels increase and hospitals become economically viable, future markets are assured and financial institutions are more willing to invest in the private health industry (BUPA, p.8).'

There is a deeper sense in which capital is ready to bring about whatever kind of medicine aids capital accumulation. The British private sector is still very small, so that the growth of an alternative to the NHS can go on indefinitely under regulated conditions (BUPA, 1978, p.10). But in America, that is no longer the case, and this difference in size actually brings out another similarity of medicine within capitalism in both countries. In 1977, Americans spent 180 billion dollars on health care, or about 9% of the US GNP. Health care in Britain costs about 4½ % GNP. In the US, that money is spent privately, for the most part; in Britain, it is spent publicly, for the most part. Yet there is a fiscal crisis in both countries. Here is the similarity in the difference. Because the enormous health care bill in the US is paid in the private sector, public controls are sought to restrain it; because it is paid in the public sector in Britain, private enterprise is sought to restrain it. Sought by whom? In both cases,

sought by productive capital *in general*. The growth of capital within health care *services* has now menaced productive capital in the US; the undeveloped state of capital within British health services opens an opportunity for capital to decrease public expenditure. But beneath the apparent difference, the basic antagonism is between productive (industrial) capital and services, whether capitalist or not.

I've left out a significant feature of the NHS, in order to detach the attempt to justify its existence from the attempt to demonstrate its uniqueness in a medical sense. The importance of the NHS as a public sector, in comparison with the largely private health care sector in the US, lies in its having established a foothold in the elimination of exchange value as the regulator of human activity and need. That simply means that it offers to society the potential for deciding what to do about health care, based on decisions about needs, rather than on the encouragement of capital — just as it already offers such a market-free decision to individuals. Actuarial calculations on individuals are simply precluded. The fact that drug, equipment and construction companies profit from the NHS, and that governments regulate unemployment and private investment levels by controlling the size of the public sector, may blunt its political efficacy, but it doesn't obliterate its political significance.

That political dimension is the other face of the pay-beds dispute. BUPA is capital in action. The Tories certainly encourage capital, but they also listen to the consultants who practice private medicine in the interstices of the NHS. And their decision to encourage an expansion of private medicine within the NHS will provoke union opposition. Although pay-beds may distract attention from the long-term trends of capital in medicine, they also have become an important symbolic political struggle between privilege and equality, standing in for capitalism vs. socialism, and fought out by consultants and government vs. public service unions. The existence of the NHS opens up precisely these foci of struggle.<sup>3</sup>

I said that I took this route in order to detach the arguments for the NHS from debates about medicine. Of course, I hope the political foothold for anti-hierarchical, anti-elitist and anti-capitalist health-care will also begin a movement for a new medicine. But we still must remain aware of the adaptability of apparently progressive movements to the expansion of capital. The NHS is a small socialist foothold in a capitalist economy, in which the public sector is regulated as much as possible for that economy. It is within these relations of services to capital, of unproductive to productive labour, that we must try to understand changes in the form of medicine. Andrew Scull (see review of *Decarceration* in RSJ8) has looked at the emergence of community care for people who previously were, or would have been, incarcerated in mental hospitals or prisons. At

the end of the day, it saves money in the public sector. What is insidious, in addition to the atrocities sometimes done to those released into no real community, is the way the ideology of community care — the science of human behaviour and treatment — somehow fits the changed circumstances engendered by the fiscal crisis. Equally dangerous is the way that community, a slogan so popular on the left for representing anti-authoritarianism, has become even more popular on the right. Like 'nature' was during the ecology movement of the 60s, community lends itself to effortless cooption.

We can find the same double symbolism in psycho-somatic medicine. It represents the disenchantment with the piece-work technology of acute medicine, but it also represents surplus value to insurance companies which can improve their actuarial calculations by improved screening of applicants for illnesses they don't yet have. Some businesses have begun to introduce preventive medicine into their contracts. One company I heard about may require counselling as a condition for continued employment. We may see, first in the US, a fault system of insurance which divides people from each other according to the illnesses they bring upon themselves. That has been suggested in Britain as well, but I suspect that it will happen more acutely where private insurance mediates between ill people and capital.

The very act of screening or regular preventive check-ups has as its other face the tacit approval of injurious working conditions and the subtle redirection of responsibility towards accidental features of personal or social life (Annesley). That's not management conspiracy; progressive programmes, such as these can be, also shift the very way we perceive our own health and illness.

Similarly, I wonder (but have no evidence) about the increasing interest in euthanasia. The fact that the age composition has moved upwards doesn't introduce old age as a novelty; nor does it bring new techniques for humanely 'easing people out'. It only brings a new problem to capital, in that old people are unproductive and the pressure on younger people to produce (most families with more than one income *need* multiple incomes — they are not a luxury) has become very intense. That may sound crude and economic. But the ideology of euthanasia stands in between, just as 'community' does in the case of decarceration. I heard a radio play recently which had the same ominous reality that *Coma* (the spare-parts surgery thriller) has. It was built upon the theme of medical centres for 'easing people out' (I think that's what they called it), and especially upon the honourableness of being 'eased out' in order to help one's children. In the end, the case in question fell through. The son of the man to be eased out, having been pushed by his wife and never happy about the decision, gave his father a bit of illicit whiskey which countered

the drug. The whole rationality of euthanasia fell apart in their reconciliation. But the eerie rational backdrop lingered. Similarly, in his television series, *The Body in Question*, Jonathan Miller ended his long march through scientific medicine with scenes of very old people sitting in lonely isolation. He portrayed them as empty hulks brought to that state by scientific medicine. With one deft stroke, he glamorized himself and medicine for their achievements, and hinted that, since they had brought the issue of life or death within human control, perhaps they would ultimately have to solve this as well. That wasn't said; there was only Miller's soothing, melodious voice stroking in the background. But it robbed dignity from these old people. It made them the material of medical advance. It made their future the problem of medicine. It implied that age itself — the old houses created by medicine — was the problem.

It did not say that the crisis of capitalism was the problem. People can now talk 'reasonably' about euthanasia; that's capital's logic. And laying stress on the expensive fight against death, rather than on the inexpensive victory over infectious diseases, capitalist ideology implies that old age is capital's gift anyway. We ought to be more humble about it, a little ashamed to have it, and ready to give it up.

### Medical Commodity Fetishism

I don't think I've strayed. I want to show that what we often take to be the contours of capitalist medicine, such as fee for service vs. a national health service, are misleading. I want to show that, beneath the obvious differences in the strategies of capital, there are deeper similarities. I want to show that the form of medicine cannot be looked at in isolation from the strategies of capital. The first premise is that capital will accumulate. The specific routes of that accumulation will vary with specific conditions.

One of those contours which may be moulded one way or another, but which is part of capitalism, is the fetishism of commodities (Young, *RSJS*). To call medicine a commodity in this sense is to suggest that social relations involving ill people are masked by objects characterized by their exchange-values. Put in a less economic way, it means that objects take on in a hidden fashion the life of a person and mediate the relationships between people. The investment of life into objects gives the objects a life of their own, so that they seem to carry on their own relationships. But objects under capitalism are not free; they relate to one another according to their value and they become, therefore, commensurable — devoid of specific quality. Now introjected, these objects, stripped of specificity

and moved with respect to one another by the law of value, reinhabit the ego — form it as a set of objects:

'The transformation of the commodity relation into a thing of "ghostly objectivity" (Marx) cannot therefore content itself with the reduction of all objects for the gratification of human needs to commodities. It stamps its imprint upon the whole consciousness of man; his qualities and abilities are no longer an organic part of his personality, they are things which he can "own" or "dispose of" like the various objects of the external world. And there is no natural form in which human relations can be cast, no way in which man can bring his physical and psychic "qualities" into play without their being subjected increasingly to this reifying process (Lukács, p.100, quoted in Schneider, p.86).'

The commoditization of social relations in medicine occurs at many levels, even in the simple instruments used by the doctor. Listen to Foucault's poetic account of mediated social relations in the newly invented stethoscope:

'The stethoscope, solidified distance, transmits deep and invisible events along a semi-tactile, semi-auditory axis. Instrumental mediation at the surface of the body authorizes a retreat which measures a moral distance; the prohibition of physical contact makes it possible to fix the virtual image of what is occurring far below the visible area; the background of modesty is, for the hidden, a projection screen. What one *cannot* see shows up in the distance from what one *must not* see (Foucault, 1973, p.164; translation altered).'

Foucault had just quoted a consultation to a young woman by one of the elites of 19th-century Paris Hospital medicine. The stethoscope clearly mediated a complex set of socio-sexual relations. Similarly, the decision to use or not to use anesthesia in 19th-century obstetrics sometimes depended upon the class relations of physician and patient. Anesthesia, physicians thought, released overt sexual behaviour which would offend the modesty of their patients, if they had known of it (Ahlborn). Whose fantasy was that? The chemical agent, the procedure and the 'technical' decision mediated this set of sex and class relations. We could multiply examples from contemporary medicine, in which social relations are frozen into objects or into routines which depend upon them: fetal monitors, obstetrical tables, many laboratory tests, tranquillisers, task-oriented nursing, standardized forms for GP consultations.

Of course, these objects are not inherently capitalist. I've referred to them to make the point that even simple pieces of equipment come out of historical settings and embody them. We could work through layers of increasing generality in medicine, looking for the specificity of social relations which is frozen into a set of objects at each layer. Take the case of the hospital, which I spoke about earlier. I mentioned that they institutionalized a relationship between social classes, in that they brought poor people into contact with doctors and nurses on a large scale. I spoke about the use of that extended contact for moral improvement. It was also used to break traditional social customs surrounding ill people (Rosenberg, 1977). But it was also in this setting that medicine reified the

disease. The medicine of the Paris hospitals, the model of 19th-century clinical medicine, was a medicine of discrete disease entities displayed in vast museums of pathology. That display and that relationship between the physician and patient, in which the medical labour process formed illness into disease, grew within a specific class relationship. It did not happen while doctors for the most part served clients from the aristocracy or the established middle class. It happened when capitalism (not just urbanization) brought the working class to doctors whose careers included hospital appointments (Figlio, 1977; Jewson, 1976; Waddington, 1973).

The sciences of hospital medicine, in addition to diagnosis, were nosology (the taxonomy of diseases) and pathological anatomy (the confirmation of diagnosis and the explication of morbid processes after death). Paris medicine had trained George Eliot's Dr. Lydgate (*Middlemarch*), whose scientific approach made him suspect in English county society. He tried to do nothing but diagnose. In a society accustomed to the service of doctors, that clinical distance, that scientificity, was out of character. Patrons negotiated; patients submitted.

In this setting, patients became the bearers of discrete pathologies — the substratum or foundation in which a disease grew according to its own laws. The biography of the ill person, the unique features of his/her life which may have included unhealthy episodes, remained only as a set of accidents which put that person in danger through exposure to pathogenic conditions. But the essential unity of person in health and illness was now increasingly subordinated to a divided person bearing a disease.

I've described this change from illness as biography to illness as bearing a disease in terms of the possibility for disregarding the patient, which the class relationships in the hospital offered. By analogy to a labour process, one could say that the new class relations allowed the medical labour process to fashion diseases from illnesses. The disease lay embedded, impure and unformed within the bodily environment, like steel within a sample of ore; it needed both separating and further processing to become the specific disease. The hospital, by continuously working on a finite set of disease products according to a standardized routine, perfected that set of diseases. The methodology combined the faith that pure forms could be elicited (not just seen by passive observation) from within a confusion of symptoms with the mass production necessary to work up a significant amount of product.

I'm not saying that doctors learned how to manufacture illness; I'm saying that they learned how to manufacture diseases along with their treatments and that, like other products, they are formed from materials, not from ideas alone.<sup>4</sup> The labour process for making diseases, like other labour processes, took on specific features from specific social relations.

Machinery made by and for capitalist labour processes could be used elsewhere, but it also embodies both in its reason for existing and in its structure, the social relations which sustain the labour process in which it is meant to be used. Specific diseases were fashioned within a specific labour process too. Part of the crisis of modern medicine is the realization that, important as those products have been in the development of the power to alleviate human suffering, they are becoming very expensive to produce and of less certain use-value. Of course, their exchange-value remains high, the skilled and unskilled workforce is still in demand, and their cheaper products can still be distributed to under-developed areas at a high profit and with a high use-value for the people as well. But people feel a new, though unarticulated, need, which is not adequately met by the commodities produced in the contemporary labour process.

I've talked about the institution in which disease as objective, discrete entities were 'seen' because of class relations which supported a new way of fashioning them. But what social relations sustained that endeavour in the first place? In part, the ideas behind the new medicine had been developing over a long period, as in the example of the circulation of the blood. We could extend the analysis I sketched there, looking for the incorporation of changing social relations into articulated ideologies of social life and similar mediated ideologies of biological life. Frank Manuel has done this for the ideology of hierarchy in a conservative period following upheaval (the French Revolution) and organicist thought in biology. John Pickstone has developed the notion of a 'liberal physiology' that characterizes the resonance between the working out of bourgeois vs. monarchical society, on the one hand, and leading ideas of the way the parts of the body interacted in 19th century physiology on the other (Pickstone, Bureaucracy). The notion of 'economy' in physiology would be worth analyzing. Keeping in mind Marx's ridicule of direct homologies, such as money and blood in circulation, we must work with sensitivity to the mediations. But I am convinced that the analysis is powerful.

With respect to discrete disease entities in particular, we can ask who wanted them and how they fit into the resolution of social forces. Feminists interested in how medicine labels deviants have argued that women's sex-specific and sex-linked class-specific behaviour gets re-constituted as diseases. Menstruation, depression, hysteria, anorexia, even motherly behaviour, all come into this category. Ornella Moscucci (Wellcome Unit for the History of Medicine, University of Oxford) is looking at one very drastic and dangerous medicalization of feminine neurotic behaviour, its localization in diseased ovaries and their surgical removal. She claims that, in the 1870's and later, a time when abdominal operations were extremely dangerous, this was the most frequently done operation, and that the British Gynaecological Association was built

largely around the claim to have identified specific female diseases which this practice legitimated. Carroll Smith-Rosenberg has argued that the hysterizing of the middle-class woman in the 19th century was a resolution of the conflicting pressures to be both free and dependent (Smith-Rosenberg, 1972). She tries to look psychoanalytically at the conditions they had to master as pathogenic, as well as at the ready availability of the medical category to give meaning — not a social critique — to the way they were said to have behaved.

For men of the emergent industrial capitalism of the early 19th century, the fear of spermatorrhea — the pathology of profligate loss of sperm — kept them to the accumulation of capital through a thrifty mentality (Barker-Benfield, *Cominos*). Working women of the late 19th century, in aping masculine aspirations, suffered from neurasthenia (Haller and Haller). So did middle-level male managers. Both the feminization of masculine labour, such as in clerical work, and the rise of a middle stratum of non-capitalist managers, characterized the emergent monopoly capitalism of the late 19th century, and neuroses and neurasthenia were among the medical markers of the new social relations involved (Schneider, pp.159-64). Willy Hellpach, a German social psychologist, distinguished between the working class and this management stratum by discriminating the hysterical form of the former from the neurasthenic form of the latter. People who, by relation to capital, were equally powerless, were nonetheless distinctive in the social relations of production. And that demarcation, seen as inherent from within capitalist organization, informed the medical categories.

I've studied chlorosis, a disease which doctors in the 19th century considered to be a discrete clinical entity with clear physical signs, in order to understand the way disease could embody social relations (Figlio, 1978). Chlorosis was a form of anaemia of young women, especially better-off women. Although it was also a disease of working-class girls, especially domestic servants, it retained a medical meaning which associated it mainly with the well-off. That association had a historical meaning, both in terms of the stereotyping of the middle-class, idle woman as an imitation of the aristocratic woman, and in terms of the class relations of the physician. The disease embodied the tensions of class formation, in which the bourgeoisie discriminated itself with increasing clarity from the working class and the aristocracy, and in which physicians overcame their largely lower-middle-class status by strong affiliation with the solid middle class. The dynamics of the process can be deciphered in the disease.

Susan Sontag has argued that upward social mobility in the 19th century was, in part, asserted through pulmonary tuberculosis. One didn't need the actual disease to qualify for mobility, but the illness behaviour, and

certainly the disease, could help. She says of Oliver Goldsmith's play, *She Stoops to Conquer* (1773):

'Goldsmith presumes that the TB myth is already widely disseminated — TB being, as it were, the anti-gout [i.e. anti-aristocratic]. For snobs and parvenus and social climbers, TB was one index of being genteel, delicate, sensitive. With the new mobility (social and geographical) made possible in the eighteenth century, worth and station are not given; they must be asserted. They were asserted through new notions about clothes ("fashion") and new attitudes towards illness. Both clothes (the outer garment of the body) and illness (a kind of interior décor of the body) became tropes for new attitudes toward the self (Sontag, pp.27-8).'

I suspect the class formation through illness is even more embedded in medical distinctions than this quotation implies. TB was a form of 'scrofula', and scrofula was definitely a working-class disease. During the 19th century, however, scrofula (TB of lymph and mesenteric mucous glands) and pulmonary TB were separated, even treated in different parts of medical books. We might look at these notions of pathology as an enactment of class dynamics. If we put together all the cases I've mentioned so far, we at least have the beginnings of a topology of social structure frozen in illnesses which, if looked at historically, could become an aspect of class dynamics.

I do not mean to neglect suffering, but rather to emphasize the material from which a discrete disease was fashioned. That material, while it included the suffering of patients, also included the process of class formation and stabilization. We can look at machinery, which has a solid, objective existence embodying an autonomous capacity to produce once set in motion, as a 'resolution of social forces' (London, p.13). That's what I'm suggesting for diseases as well. Diseases, like technology and like knowledge, are constituted by highly mediated social forces.

This is all about the building up of mediations of capitalist social relations, so that they later reappear as discrete diseases fashioned from raw materials. Let me turn now to one of those mediations whose institutional form is still with us. I'll call it 'the insurance mentality' (Figlio, *The Role*). In the context of capitalist social relations, illness means lost production. It also means an undisciplined 'porosity' of labour, i.e., interrupted and uneven working (Palloix). Leaving aside the efficacy of medicine to guarantee a healthy labour force with greater efficiency, we can concentrate on the effect of reconstituting illness as discrete diseases. Paraphrasing the terms of the British National Health Insurance Act of 1911 (sect. 8(1)), a working person could be away from work on two grounds only: incapacitating illness or voluntary absenteeism; and illness meant a certified physical disease. That Act consolidated the hundreds of workers' sick-club programmes and rationalized their operation on a national scale. That's a gross simplification of a complex history

(Gilbert), but it is only to make the point that 'voluntary absenteeism' vs. discrete physical disease is an insurance concept built on the principle of sick-leave from work.

Looking at the doctors' role in running this act, we can learn something about the class relations of production and the place of medicine within them. The legitimacy of a claim for paid sick-leave lay in the hands of the insurance commissioners of the particular insurance group involved. (The insurance policies were still offered by insurance societies, though the Act required that they now be 'approved'.) Doctors would often simply state that a claimant was unwell and unable to work. They would, that is, often become the claimants' advocates. The insurance commissioners, on the other hand, asserted that they wanted from the doctors, not an evaluation of the patient's well-being, but testimony as to the presence of a specific disease (Departmental Insurance Committee, par.127). In today's context of a rather haughty medical profession, we forget that doctors were, for the most part, lower-middle-class practitioners with a defined role.

More than today, 19th-century medical practice must have included a lot of undiagnosed illness. Even today, many consultations do not lead to a diagnosis, and most unwell people do not go to a doctor.\* But that misses the point. To the extent that the social relations of production could penetrate into everyday life, to the extent that illness became manageable by those relations, it had to be reconstituted as disease. And to the extent that recent holistic tendencies have been attractive to capital, it is not to restructure that model of illness, but to extend its scope and hegemony.

In case you have lost track, I've been talking about illness reconstituted as diseases. That process of reconstitution is a form of commodity-like fetishism, in that the diseases then mediate social relations; they reverse the order of production, so that they not only appear to be socially rootless, but also appear to regulate social life. That fetishism reinforces the liberal view that progressive change means striving to attain equal access to consumption, i.e., the equal distribution of commodities. In the case of medicine, it means what Illich called 'equal access to torts' (Illich, 1976, pp.237-44) — the right to equal access to medical care. Within the commodity-controlled market and mentality, that means equal access to the medical labour process which reconstitutes social relations as medical commodities. Put less abstractly, it means forfeiting the potential awareness and political processes by which the essential relationships between morbidity and production, morbidity and social structure, can be discovered. It means accepting a liberal notion of need, in which all personal

\* The so-called 'clinical iceberg', i.e., that medical practitioners have only seen the tip of an iceberg of sickness; see Tuckett.

needs and social needs are commensurable — reducible to a common set of variables which can be measured along scales of 'greater' or 'lesser'. Qualities reduce to quantities, so that an allocation can be computed and formal equality sought after.

The NHS undercuts some of that commodity-orientation, by appearing to secure equal access to facilities. There are regional differences and administrative decisions which do prevent equal access to medical care, but that inequality isn't really the issue I'm after here. The commodity mentality still reconstitutes illness as isolated sets of disease 'commodities' which set in motion the movements of other commodities (services, equipment, supplies, drugs and even the basic direction of industrial strategies for future production). The commodity market sustains an enormous volume of business and production, sustains a belief in humming efficiency of the government and social machinery, sustains the faith of vulnerable people that this machinery (and more of it) is their only hope for relief from suffering or for continued life, and sustains the belief that illness is an accident of exposure — a risk — and not an integral part of their social fabric.

Very few of us will overcome those beliefs on our own. And those who seem to do it will completely miss my point if they take me to advocate discarding scientific medicine. To free ourselves of commodity production by breaking the machines is Luddism, and that holds for medical commodities too. To release production from the commodity market is not the same. We don't know what that means; we can only work 'prefiguratively' in theorized concrete steps (Young, 1977). Somehow that must increasingly include the recognition that the organization of production entails morbidity both directly and through its effect on social organization away from the point of production. To allow morbidity to appear as a problem of differential incidence in a stratified society is similar to believing that social inequality is unequal opportunity and a poor distribution of the proceeds of labour. It denies the structural limits within which people are made unequal. But even the very notion of distributing equally the proceeds of labour presumes a standard measure of need and of contribution to the social product. Even if 'fair distribution' replaces equal distribution, the whole process retains the structure of an autonomous commodity market, with measures of necessarily measurable variables (Marx, 1875 (1959), I.3 with critique, pp.115-20). Our task is to make the distribution of things by exchange-value an irrelevant concept, by defining needs in a directly social way. The meaning of 'directly social way', and the path to it, lie in practices, not in predictions (Heller).

In the case of health care, needs irrelevant to production will always remain. Marx simply assumed that health care was a deduction from the

social product prior to meeting any remaining needs (Marx, 1959, p.117). Illich grants those needs too, saying that the limitations should be that no individual health need should prevent anyone else making the same claim, and that there must be limits to the overall social deduction to meet them. One might say that precisely those decisions are the crucial and apparently insoluble problems of the NHS. We have very little experience of needs in an uncommoditized form, and no way of imagining a world without them. We do know, however, that commoditization of needs has severed the social and physical organization of production from 'society' and isolated our experience of illness from our work. Politicizing illness, therefore, requires that we begin to incorporate illness into a collective framework within groups who share concrete collective struggles.

### **Illness as Collective Practice**

In this final section, I want to combine two themes. First, I want to develop what I started at the end of the last section — that illness has to be seen in the context of the mode and social relations of production (i.e. in its determinate aspects within a structured society), not just within 'society'. Second, I want to refer that strategy to the general theme of this paper — the nature of capitalist medicine. I can state what I mean in one sentence: Capitalist medicine reconstitutes illness as disease in 'society', where 'society' is a grab-bag of accumulated accidents, and it responds to it as if it were either an individual incident or, at most, a skewed level of incidence due ultimately to a distorted distribution of wealth. It's about risks, not about determinate aspects of society. Remember the quotation from Marc Renaud which said that modern medicine epistemologically eliminated society from the etiology of disease? That's true. But medicine also acknowledges society in a way which is as dangerous as this epistemological silence. Social causation of illness, when it is recognized, usually means environmental hazards — not just the obvious case of pollutants, but rather the undifferentiated backdrop in which illness occurs by exposure. Illness becomes a problem of incidence — the number of people who contract this or that pre-existent category of morbidity because their social position puts them into a measurable risk grouping. The inward relations between what people do and what they suffer — between their social existence as organized activities, needs and wants, and their illnesses as intrinsically related morbidity — is lost in a notion of illness and a methodology which ultimately presumes that illness is either accidental or self-inflicted.

The opposite of 'accidental' is not 'intentional'; it is 'essential', in the sense of having an inward, meaningful quality. Marx meant to unmask the essential relationships between production and consumption,

between capitalist ownership of production and alienation, between commodities and needs, between labour and labour as a commodity, etc. He sought 'internal relations' (Ollman), not mechanical ones. And that had nothing to do with correlations between one category and another, but with the essential nature of the relationship. That's what I mean by saying that 'society', when it is not epistemologically absent from the etiology of diseases, usually enters only to deny an 'essential' place in that etiology for what people do within determinate limitations of capitalist society.

Michael Schneider speaks of this 'blind spot of all bourgeois theories of illness':

'All bourgeois theories of illness, whether it is a question of psychoanalysis, of psychosomatic medicine, factory psychology, labor medicine, or the newer medical sociology, contain the same basic flaw: they assiduously overlook or euphemistically circumscribe the primary social breeding ground of illness, namely, capitalist relationships grounded in work (p.166).'

Schneider doesn't say just 'work', he says 'capitalist relationships grounded in work'. That's an important distinction, because one could argue that there is an intense interest in relating work and health at the moment. But that interest centres on relationships between work and health which appear to be accidental. Illness, even occupational disease, is implicitly conceptualized as a problem of the accidental exposure to risk, not of the inward relationships between work set in its capitalist context and illness.

That accidental relationship of illness to work was retained in the notion of workmen's compensation. The British Workmen's Compensation Acts of the 19th century, which covered accidents at work, were extended to include disease in 1906. The new Act specified that compensatable diseases had to be *accidents* of work, i.e., they had to be the result of an exposure to risk which happened to occur at the work place and nowhere else. It acknowledged a disease whose occurrence approximated to falling and injuring oneself in an unforeseen way. Only under very tightly controlled conditions could diseases within the nature of work be allowed. And those conditions, limited to seven listed diseases, comprised, for the most part, poisonings — conditions in which the risk of exposure, rather than inward relationships, stand out.\*

One might interpret the Act as containing, not just the direct cost to employers for occupational morbidity, but also the charge that there was morbidity in any form other than unforeseen risk. In the debate before the Act, the government decided that any further acknowledgement that there were illnesses in the nature of work should lead to national ins-

\* There are now 51 listed diseases, plus a special category for pneumoconiosis (Hunter, pp.208-216).

urance (*Brit. Med. J.*). At one level, that was progressive, in that it showed the acceptability in principle of removing medicine from the market place. But it also showed the desire to move quickly to socialize the costs of capital's pathogenicity should the issue arise and, in effect, to displace the location of the problem from production to society. They spoke of the hopeless tangle of adjudication should the Act have been extended, which was no doubt an accurate perception. But we have to look behind the practical difficulties of administration and ask what forms of illness in capitalist society the Act sustained and what forms it suppressed. And when we turn to the present to ask what forms of illness are allowed today, we have to ask how it could be that so much attention is given to excluding or proving a definitively occupational cause, when 'a welfare state that ungrudgingly met the needs of all people, whatever the cause of their misfortune, would free all this energy and skill for the real task of preventing illness' (Kinnersly, p.174).

A recent article in *The Lancet* criticized an American study which had argued that occupational cancer accounted for a larger proportion of all cancer than people had previously thought. It said, for example, that 13-18% of total cancer was caused by asbestos (while hitherto, the estimates of total occupational cancer was 1-5% and, in a recent study, 10-15%). *The Lancet* article criticized the American study on grounds of carelessness, saying that it had not estimated the degrees of exposure and social-class gradients of morbidity. Perhaps these non-occupational factors might have correlated with the levels discovered. I have no comment on whether or not the American study was careless; I'm more interested here in the strident tone of denunciation and in the hypothesized alternative explanation.

*The Lancet* said that non-occupational causes, such as cigarette-smoking, which has a marked social-class gradient, might exemplify an explanation superior to what the Americans offered. Since lung cancer makes up about 40% of all cancers, it could affect total cancer rates by 9% (assuming, I guess, the attribution of about 22.5% of lung cancer to cigarette-smoking). Using figures on social-class gradients and smoking behaviour, it argues that manual workers might have 22% more lung cancer risk than administrative or clerical workers.

First of all, what is the point of arguing down the levels of occupational cancer? Is it the interest of science, constantly seeking more precise explanations, or perhaps dismay at inelegant work? Or is it aligned with the ideology of personal responsibility which the DHSS has promulgated through its consultative document, *Prevention and health: everybody's business*. Here, the discussion of the variations in cancer incidence follows the discussion of social class gradients in mortality, morbidity and sickness-absence, and of environmental differences. The cancer vari-

ations are then discussed in terms of sex and regional variations. They exclude — are epistemologically silent on — occupational cancer, and conclude (referring to the regional/sex variations) that ‘. . . with the exception of cancer of the lung, we do not know of any removable cause for these variations’(p.54). Why should there be a removable cause for regional variations, rather than for occupational variations? It sets up the problems in terms which refer to the ‘environment’, acknowledges some pollutants, and refers to the one case in which individual behaviour can be implicated.

I'm not suggesting a conscious collusion to disregard occupational illness. I'm talking about a patterned perception of the nature of illness which affects both our everyday attitudes and the construction of scientific problems. It structures the sense of the right questions and the right actions. Ultimately it serves a class interest, but it presents itself as universal. Prevention is everybody's business, but only as an objective for each of us facing illness and death as individuals, reaching out sympathetically to other individuals. As a prescription for social action, it feeds on the illusion of a common offensive against a common enemy, in an attempt to cut across the awareness within groups that they may suffer from social structural, not individual, causes of illness. As a consequence, the unaccountable power of those interest groups which brought about the pathogenic conditions in the first place is consolidated.

The second point about *The Lancet* article is that, like the DHSS document, it dichotomizes occupation and environment. Rather than work towards a unified view of the common constitution of society in its production and non-production aspects, it sustains their separateness. While I'm sure that occupational medicine is constructed that way more for analytical clarity than for consciously devious purposes, I also think that ultimately there is no clarity in such a dichotomy. It will allow neither a medically nor a politically viable solution. Disease won't disappear under socialism, but the point is to go beyond blaming (or defending) ‘victims’, by developing collective resistance to the conditions of our passive victimization.

The final point about *The Lancet* article concerns its use of social class to account for variations in cancer rates. The ‘science’ may be valid, in the sense that a respected body of literature with a recognized methodology has produced research on the relationship between definitions of social class and illnesses. But social class, except in the dynamics of group social relations structured by specific forces, is an empty box. Like ‘environment’, it can be put against occupation as a relatable, but not inwardly connected, category. And because illness belongs to the preexistent categories of social class, rather than to people in common pathogenic

conditions, illness remains accidental. It remains a problem of risk and of exposure, not of internal relations.

The DHSS document uses an interesting phrase which suggests the ultimately accidental and individual nature of disease. Speaking of social class differences, it says:

'The table shows how men in the professional groups (Social Class I) have better mortality records than almost all other groups for all diseases while labourers and unskilled workers (Social Class V) have a worse record in every category (p.51).'

Put 'mortality record' with 'track record' as an expression for the success or failure of initiative and perseverance. Medical sociologists have even taken up the notion of 'illness career', which carries overtones of career from the sociology of work — a kind of personal trajectory within categories of structured expectation (Gerhardt). Records and careers have to do with competitive performance. It may have been a casual choice of a word, but it certainly was a curious choice, either to have made or to have left in after the first draft.

I'd better come back to the point. The separation of society or environment from occupation complements the separation of consumption from production. It puts the problem into a sphere of distorted equality or opportunity, rather than into the mode and social relations of production. It isn't just a question of getting sick at home vs. at work; it's a question of how the re-locating of the ills of capital, including the illnesses of its people, outside the heart of capital occurs. I don't think that the blind spot of industrial pathology (Schneider) is just over who will pay for illness. Were the epistemological silence on illness and capitalist organization of production suddenly to shout for recognition, it would threaten, not just liquidity, but capital accumulation. It would make an inward connection precisely where there is an epistemological silence now — in the collective awareness of the pathogenicities set in motion and sustained by accumulation. At present, it remains absurd — effectively an impossibility of formulation — to see in our working careers the basis of our illness careers.

We can draw an analogy from the labour process. At one level, the interest of capital requires that efficiency constantly increase. The organization of the labour process becomes, in such a view, a simple matter of continually seeking more efficient ways to do the job. It is not a problem of social relations, nor of antagonistic interests; it is a cooperative, mutually advantageous process of gaining a competitive advantage. But the organization of the labour process and its restructuring have another feature, or can be seen from another point of view. From this perspective, capital needs a labour process which undermines by its very design the potential threat to efficient production posed by any collectivity. Beneath

the 'objective' principle of efficiency lies the issue of social relations of production, and these relations reveal an antagonism between capital and labour. The abstract is in the concrete. Capital wants efficiency and labour wants better pay and conditions.

What about capital's attitude towards illness? Well, at one level, a healthy labour force is a productive labour force. That has been a standard interpretation both among liberals and marxists. In this view, as in the organization of the labour process, capital and labour share a common, productivist interest in improving health. Critics may trade on the callous calculation of health = productivity but, within the goal of production for competitive advantage, it makes as much sense as it does to train athletes strenuously. But from another point of view, analogous to looking at the social relations of production, rather than at the techniques of efficiency, a healthy labour force might be supremely unproductive because that health carries with it a collective awareness of the inward relations of capitalism to illness. The epistemological silence on the structural relationships between the capitalist organization of work and illness is analogous to the implicit process of fragmenting collectivity in the labour process.

Illness is contained in the wage relation in a society in which consumption mediates well-being. One can measure and compare, and use it to demand a higher social wage. It is treated outside production, just as other needs are met outside production. It becomes an issue of equity, just as purchasing-power does. Choosing to 'own' an illness becomes a way to express a grievance — an inarticulate outrage against unfairness. The DHSS document says that the boundary between the high morbidity of lower social classes and the expression of job dissatisfaction is unclear (p.53). People use illness as an excuse to stay off work. But the other side of this story is that they stay off work alone. It is a protest, in the form of having a grievance. It is not a collective awareness of capitalist pathogenicity, and even less is it a collective struggle. Although lost days through sickness are said to cost a lot, they do not threaten capital. Indeed, they reinforce a wage-like relationship between capital and labour, in which the only issue under dispute is what it will cost to avoid truculence. And at the more extreme end, '... a constantly increasing defection from work through illness makes the creation of a potentially revolutionary army of unemployed — in a period of depression — very difficult, if not impossible' (Schneider, p.186).

The ill are an invisible reserve army of labour, a buffer for capitalist expansion and contraction, enumerated and paid out of a budget in such a way as to separate them from the self-consciously unemployed. Their political strength, their political consciousness, is muted. Polack argues from calculations on days lost through illness in America that 'American

civilisation could not support the total suppression of tuberculosis without putting in question its economic structure' (Polack, quoted in SPK, p.147, n.10). That's another side to the ominous tone in which the Office of Health Economics announced '... on the national level absence attributed to sickness has been seen as one of the factors contributing to the relatively poor performance of Britain's economy in relation to other industrial countries' (p.3). Could this be a curious piece of false consciousness? Putting these statements together, one could form the opinion that decrying the levels of sickness absence and needing them for economic and political stability go hand in hand. A German study shows how the levels of sickness absence reflect the relative strength of capital and labour (Schmoll). When employment levels are high and labour's position is strong, sickness absence rates are high; during recessions, rates are low — presumably because people who stay out of work get sacked first, even if their excuse is legitimated. The issue is clearly complicated, but the hints thrown up do show that illness is a potential field of struggle.<sup>5</sup>

These examples spoke of illness on the margin of the labour process, acting to regulate the relations between capital and labour in a depoliticized form without potential for collective action. Some people have begun to ask about the political nature of illness itself, i.e., whether its very presence is itself a political statement:

'As long as the working class does not rebel against these new and intensified forms of exploitation, heart, stomach, and circulatory diseases of individual workers will rebel for them. Even though the worker may still "go along", his circulation, in any event, will not. Even if he says, "actually I feel all right," his stomach ulcer will prove the contrary (Schneider, p.183).'

That suggests that illness is an alternative to struggle, that the failure to organize and collectively to resist exploitation is itself pathogenic.

Schneider puts the social relations of production at the centre, while capital might at most concede that some procedures carried with them a higher than foreseen risk of exposure to a pathogenic condition. Chronic illnesses, being more difficult to associate with an accidental feature of a work process, implicate the work itself in a way that acute, infectious illnesses do not (Berliner and Salmon, p.37). With the decline of acute diseases during the 19th century, and the increase of chronic illness, the political nature of illness with respect to the capitalist organization of production began to emerge (Figlio, 1978). Nervous illnesses are one aspect of that refinement of the political character of illness which has occurred as acute disease, and the hygiene measures to combat it are superseded by the chronic illnesses of more advanced capitalist society:

'[In the great modern factory at a high level of organic composition of capital], one can demonstrate, on the one hand, the diminution of traditional injuriousness, and, on the

other, a collection of organizational techniques of scientific exploitation, and therefore of new contradictions between workers and capital, which multiply psychological discomforts, neuroses, etc. . . . It is therefore true that the struggle of workers against the traditional injuriousness can lead to an amelioration of conditions of hygiene, but that engenders almost immediately an acceleration of pace and new situations of organization which necessarily renew the struggle for health at a higher level, where the mode of production of the capitalist plant is itself attacked. That contributes to explaining why, at the same time as the imposing increase of nervous and psychosomatic troubles, the most advanced workers' struggles in the confrontation with the problem of neuroses thus directly become political struggles (Jervis, p.89).'

Jervis adds another dimension to the politics of illness. Like Schneider and the Socialist Patients' Collective of Heidelberg (SPK), he takes neurosis to be an internalized alternative to political action. 'Neurosis is a form of political false consciousness. If it can be considered a moment of contradiction, a step forward in relation to acquiescence and resignation, it is also — mostly — a moment of blockage, an obstacle on the path of seizing hold of collective consciousness of the political problems in play, an individual suffering which facilitates neither clarity nor action (Jervis, p.98).'

But from the earlier quotation, he seems also to say that the politics of illness will actually be driven to the surface by the sharpening of contradictions within advanced forms of production.

Having come this far, however, Jervis conflates two notions of collectivity in a way which reveals an essential problem for prefiguring a socialist practice. In the first sense, he sees personal suffering and collectivity as alternative forms of consciousness — really two states of being. The former is passive; the latter is active. The former suffers; the latter is in movement. The fragmented state refers to individuals who are indeed a group in their common, but nonetheless individual, relationship to capital and perhaps in their common work experience, but their communication — their joining — is muted and perverted. Statisticians and occupational and community physicians might see them as a group in terms of their shared social class indices and even their shared morbidity. But there is no solidarity — no collectivity — in this state. They participate only through communicating, in their negativity and their illness, a common, but separate, grievance. The collective state, on the other hand, senses its own solidarity and its common telos, and communication through illness is replaced by a life-giving, direct participation.

Next to this view, which I have drawn out from Jervis, he puts another, more mechanical one, as if it were the same. In the second quotation, neurosis is an obstacle to political action directed towards removing the causes of sickness. That is, it prevents the general struggle for improved conditions. In this sense, collectivity approximates co-operative, but not collective, action against a recognised grievance.

I may be making far too much of a distinction which Jervis did not intend.

If so, I apologize for the distortion, but not for the issue itself. Whether or not he intended this distinction, I think there is often a conflation of the two senses, so that each trades off the other without our knowing how actually to bring them together. The former carries the optimistic feeling of an inward resonance among people bonded in a new way; the latter offers down-to-earth struggle which we can all recognize. The former invigorates the latter, while the latter incorporates the former's ill-defined communitarianism into a programme.

I'm not sure how to proceed, because I'm not sure what solidarity and collectivity really are. But I think it is crucial to hold to the distinction between them and joint or co-operative action. Unlike the case of conflicts over wages or conditions, politicizing neuroses and, ultimately, illness, is about the interiors of people. It speaks, or reaches, to that area which is outside the possessive individualist definitions of equality and need. It is inadequate to politicize neuroses in the sense of repairing personal suffering through successful collective refusals in the struggle for changed conditions. That is important, as far as it goes. But what it misses out is the seeding of a positive socialist practice, in which needs, by no longer being commensurable, become totally individual. Psychoanalysis, though it has no sense of collective action to replace private struggle against internalized authority, does have a sense of wholly personal histories and needs. That historical commitment, broadened by an awareness that its own concepts are also historical, has somehow to be preserved in any collective lifting of neurosis and illness. I'm not even arguing for psychoanalytical politics, but for the discovery in practice of collectivity as a positive, prefiguring entity.

The alternative between neurosis and politics, which draws symptoms out of contemporary work experience and scorns the psychoanalytic recovery of their origins in early development, answers the desire for action. It also sustains the desire to put the causes of suffering onto a recognizable enemy and, in doing so, encourages a personalization of grievances. Those tendencies are ultimately reactionary. They cast aside what a politically informed psychoanalysis may yet yield as a basis for positive practice — a grasp of the internalized barriers to, and the ingredients of, human collectivity. And they also detract from the historical and structural analysis of that epistemologically silent space which ought to speak of sociogenic illness. This is not only about people in society, but also about society in people.

To the extent that capital controls through its medicine, that medicine must be opened to struggle. That doesn't mean that diseases will disappear under socialism. Their unequal distribution may decline, and the determination to reveal their meanings might intensify. As a focus for political activity, we must see what illnesses, whose meaning is now set

within a bourgeois medical framework, signify about socio-economic and political organization, and what they offer to socialist action.

Of course, we may discover that collective action does reduce morbidity. If we do, so much the better. Thinking about disease in new ways, especially in socio-psychological ways, does supplement our common-sense experience of ill-health, so that both can give sureness to what might otherwise remain barely credible clues that something is not right. That subliminal trace of a disturbance has to be intensified and clarified in concrete situations.

## Notes

1. Brian Hurwitz adds this note: 'Routine therapy for some cancers and leukemias in many centres now involves administering a dose of cytotoxic (a cell-killing drug) several times the known lethal limit for humans, followed several hours later by a rescue dose of anti-cytotoxic to neutralize the effect before too much damage is done to the healthy tissues. In this way it is hoped to kill as many tumour cells as possible without killing the patient.'

'It seems to me that this therapy, which I have often administered intravenously, in the careful titration of poison vs. rescue — it is known as rescue therapy — best illustrates the metaphor of battle/struggle in the quotation.'

2. In another paper, which I discovered while I was revising this article, Eyer (1977) replaces spontaneous community in the abstract with a dynamic relationship between solidarity and the disruption of solidarity as a moment of class struggle. The business cycle, in this view, is a cycle of changing power relations between capital and labour. As unemployment rises and falls with the cycle, so does the power of capital over labour and the difficulty for labour in embarking on, or sustaining, a strike. Strikes mean collective action — solidarity — and as strike frequencies rise with the upswing of each business cycle, mortality rates drop. Eyer claims to account for 89% of the cyclical variation in mortality by changes of social relations in class struggle, i.e., strikes, marriage, overwork, migration, divorce, unemployment, and alcohol and tobacco consumption.

3. Union action against pay-beds goes beyond the usual concern for pay and conditions to challenge social relations within medicine and even the right to manage. But at the same time, the form of the action remains a refusal, rather than an initiative towards a new medicine. I'm not saying that this particular dispute should or could be anything else and I am not

denigrating the political significance of its anti-elitism. I only raise it to differentiate what it is from what it is not. It seems to me that one of our hopes for the future might be that workers will increasingly over-step trade unions' traditional prerogatives, but not only to challenge hierarchical social relations and their consequences — private medicine, in this case. Might they also challenge the forms of medicine embodied within the social relations they challenge? There are few models for this kind of action that I know about (and I welcome more information). One is the Lucas Aerospace initiative, in which workers' reactions to redundancy included proposals for socially useful products (Institute pamphlet). The other, more an inter-professional dispute between medics and social scientists, involved the sacking of a research department for addiction studies at the Institute of Psychiatry in London (Triesman). The sackings simultaneously asserted medical authority within the social hierarchy of the Institute and medical hegemony over the actual conception of addiction and the way it is to be studied.

4. I don't mean fatuously to neglect that illness causes suffering and death. Rather, I'm trying to find ways to focus upon the specifically capitalist forms of perceiving, treating and assigning responsibility for illness. I'm also trying to understand the way these social and epistomological features form into an integral part of social relations in general, e.g., between social classes, employer and employed, the state and the 'people'. I felt at first that I was straining at the limits of credibility, in speaking of medicine in terms of a commodity-producing labour process. Now I discover that language to be routine in health economics:

'One of the novel features of the [health capital] model is that individuals "choose" their length of life. Gross investments in health capital are produced by household production functions whose direct inputs include the own time of the consumer and market goods such as medical care, diet, exercise, recreation, and housing. The production function also depends on certain "environmental" variables, the most important of which is the level of education of the producer, that influence the efficiency of the production process (Grossman, p.225).'

We're all mini-capitalists, each producing our own health with our own labour process, interlocking with all the other forms of goods production which we need and can buy on the market. Don't laugh it off; how deeply are ideas like these, and their consequences (where could any social, let alone socialist, medicine be conceived within such a framework?) embedded in our thinking and practices? We have to understand, not just the atrocities, but also the limits to what we can imagine.

5. Peter Taylor rejects such a relationship between sickness absence and unemployment levels for post-war Britain (also, Taylor and Pocock). He dismisses the charge that workers are motivated purely by money, and, therefore, will exploit social security if it pays to do so. But, in following

that line of reasoning, Taylor implicitly accepts the framework of an imaginary critic of the social services, rather than forming a political analysis of the forces expressed through a relationship between unemployment and sickness absence. Schmoll, for example, sees it as active resistance to exploitation and active struggle for necessary recuperative time off. The relationship reflects the contestation of the notion, and measure, of health. I've casually compared sickness-absence rates, in the form of days of work lost in aggregate, with unemployment levels in Britain from 1962 to 1975, and I do find them varying inversely.

# SEVESO:

## Safety in Numbers?

**An Essay Review of Laura Conti, *Visto da Seveso: l'evento straordinario e l'ordinario amministrazione*, Milano, Feltrinelli, 1977.**

**Gianna Pomata**

Comments within the Left on events in Seveso have generally focused on the causal relation between capitalism and industrial pollution. The episode has been taken as a further instance — as it certainly is — of the link between capitalism and ruthless exploitation of people and environment. It is easy — and right — to lay the responsibility for Seveso's catastrophe at the door of the multinational Givaudan-La Roche. What is less easy, even from a leftist viewpoint, is to assess the responsibility of science in the case. It has often been said, for example, that if La Roche had paid due attention to the scientific reports on dioxin's effects, as observed in previous industrial accidents, that is, if scientific knowledge was not subjected to the distorting influence of capitalist interests, then the catastrophe, or at least some of its worst consequences, would have been avoided. As far as this is a typical statement, the case of Seveso does not seem to have shaken the common belief in science as a body of true statements, unbiased by social interests and therefore entirely reliable. Comments in the Italian Left on Seveso, particularly those from within the Communist Party, have consistently combined a strong emphasis on capitalist responsibility for the disaster with a view of science as the only reliable cognitive basis of effective protection from pollution. Contrasted with the corrupting influence of capitalism, science still keeps, even in the eyes of some people on the Left, a halo of purity; it is seen as the only thoroughly clean element in a polluted world.

Taken against this background, Laura Conti's book (*Seen from Seveso: an Extraordinary Event and the Ordinary Administration*) is refreshing. The book gives a detailed account of the first year at Seveso after the accident of July 1976. It relates how, month after month, the public authorities and the community reacted to the disaster. As a medical doctor and a representative for the Communist Party in the regional government of Lombardy, the author has had a first-hand experience of

the events and she relates them in the unpretentious and frank style of a work journal, without the faintest shade of official tone. The merit of the book lies principally in the quality of the information it gives about the role of science in the management of an industrial, ecological catastrophe. The book has no theoretical ambitions; it does not debate in general terms the relations between science, capitalism and industrial pollution. When theorizing, the author very often resorts, more or less naively, to the model of the capitalist 'abuse' of science. But in spite of this theoretical allegiance, the book is full of observations about the role and responsibility of science itself in the mismanagement of the Seveso disaster, observations which — as I will try to show — directly contradict the 'abuse' model.

This is not due to the author's empiricist adherence to the facts but to the autobiographical viewpoint from which she chooses to tell the story. Laura Conti writes from the starting point of her own experience. She frankly describes her bewilderment when confronted with contradictory scientific reports, her feeling of helplessness in the presence of a danger that science cannot assess with reasonable certainty, her frustration and anger at safety measures taken in the name of science but devoid of practical meaning. The dramatic significance these events have had in her life, shaking her trust in her double role as scientific and political officer, makes her able to observe and relate episodes which cannot be referred to an 'abused' science but to a science fully responsible for the social processes it enacts. Her commitment to understand and communicate her own experience, expressed by her biographical approach, is stronger than her allegiance to the 'abuse' model of science. From the viewpoint of a criticism of this model, therefore, I found her book rich in useful suggestions and ideas, which I shall try to discuss in this review.

The book is also intriguing and moving. Together with her own experience, Laura Conti relates, with great emotional and intellectual involvement, the experience of the people of Seveso, the story of a disrupted community. We learn how the people of Seveso have tried to adapt to the new situation created by the catastrophe, trying to survive as a community rather than as scattered exiles. We are offered a penetrating account of the way these people have reacted to the scientific definitions of pollution, danger and risk and to the safety measures taken by the authorities; how they have accepted or rejected these definitions and measures according to their compatibility with their own effort to keep some form of communal identity. We learn that the people of Seveso have both experienced the scientific definitions of danger as unquestionable, and sceptically rejected them as meaningless. The book, therefore, gives very interesting evidence on the general question of the hold that scientific ideas can have on people in a dramatic environmental crisis. It

can fruitfully be read as an anthropological description of a community facing the conflicts and tensions between scientific and popular attitudes towards industrial pollution.

Since I cannot presume that the events are generally known, I will briefly review them. On 10 July, 1976, an explosion at the Icmesa factory in Meda (a small town near Seveso, Lombardy) released a cloud of poisonous gas over the town of Seveso. Some days after the accident the multinational Givaudan-La Roche, owner of the Icmesa factory, communicated to the Italian local authorities that the gas released by the explosion contained a highly poisonous chemical, dioxin, of which the pathogenic effects on animals and humans, even at very low levels of exposure, were already known from previous industrial accidents. Dioxin was also held responsible for genetic alterations both in animals and people. The population of Seveso had been exposed to it for days without any caution. When the first evacuation measures were taken, a few weeks had already elapsed since the disaster.

The first months after the catastrophe, according to the narrative of Laura Conti, were months of general bewilderment. Gathering information was extremely haphazard and arbitrary, even for public authorities. The scientific reports contradicted one another and were by no means clear. The reports issued by the local government did not state their scientific sources — an attempt, Laura Conti suggests, to avoid mentioning their chief source of information, namely, the La Roche laboratories. For months no official statement on the nature of the chemicals issued in the Icmesa's explosion was published; when, at the end of July, a report was finally issued by the Lombardy authorities, it defined dioxin as 'a heretofore unknown gas'. At a session of the Lombardy Regional Council, Laura Conti was given, by responsible officers, oral information about the level of dioxin in the polluted area. She found that the information did not correspond to the data contained in the official written report and she asked for clarification. She was told that the oral information was correct but no-one bothered to rectify the error in the written report.

While confusion and inaccuracy spread, great emphasis was being laid on the objective scientific criteria used to define and delimit the polluted area. Seveso was soon divided into three zones according to the degree of dioxin found in the soil, in objects and in vegetation: a Zone A, where the dioxin level was higher than the amount declared 'acceptable'; a Zone B, with a degree of dioxin pollution lower than this amount, and a Buffer Zone, where dioxin could not be detected with the available scientific instruments. The map of pollution was apparently based on the most objective of procedures, scientific measurement.

Safety measures in relation to the population were taken accordingly: Zone A was completely evacuated; Zone B was subjected to 'precautionary measures', including suspension of work in factories and shops (but not in houses) and removal of children and pregnant women during the day (they were allowed to return to their homes for the night). In addition, all the people of Seveso were strongly recommended to take scrupulous care in their personal hygiene.

So people kept washing themselves and housewives went about their daily work, cleaning and dusting somewhat more carefully than usual. However, both the map of pollution and the safety measures were based upon a lie: no amount of dioxin can be scientifically declared 'safe'. In fact, the map, the safety threshold and the evacuation had chiefly a political and psychological meaning. Laura Conti writes:

'While science said that with dioxin there can be no safety threshold and that therefore a complete evacuation of the whole area was the only adequate measure, more complex reasons, both political and psychological, suggested that somehow a threshold, *any threshold*, had to be fixed. As local governmental authorities we had to choose between the pathogenic effects of dioxin and the pathogenic effects of evacuation. A mass exodus would have been experienced by the people not as protection but as persecution. Since there is neither a way to detect the presence of dioxin in the human body nor a way to measure the level of exposure to dioxin in people, and since the pathogenic effects of dioxin are nonspecific, we could not announce a new disease, we could only forecast an increase in mortality, too abstract to be perceived by the people as a concrete danger, worth fleeing from. We could only say that "one day" (and we did not know when) "somebody" (and we did not know who) would have fallen sick (and we did not know of what). For the people it was as if we knew nothing at all (pp. 55—56).'

She is describing here her frustration as a scientific officer (a medical doctor) confronted with uncertainty and impotency, and forced to resort to a compromise between her scientific conscience and her political role. In her frank admission of the entanglement of scientific and political motives in the administrative management of the catastrophe, she gives an important clue to an understanding of the people's reactions. These reactions are the most important and strange aspect of the Seveso episode: the people refused to believe in the danger, or rather oscillated between anxious requests for scientific information and reassurance on one side and utter scepticism, accompanied by hostility to, and non-compliance with, the safety measures, on the other.

According to Laura Conti's report, scepticism and indiscipline developed gradually as a reaction to the mismanagement of the situation. At first, people asked anxiously for scientific information. They wanted to be 'tested'. They wanted to know the exact figures of dioxin level in their town, in their houses, in their own bodies. They wanted to know the probable incidence and the nature of the diseases and of the genetic alterations foreseen as a consequence of their exposure to dioxin. When

they realized that the official reports were contradictory and unreliable, they autonomously wrote to international institutions asking for scientific information.

But a discrepancy arose very soon between popular and scientific criteria in the delimitation of danger. The scientific and official criteria were, as seen before, the measurement of dioxin level and the determination of a politically motivated 'safety threshold'. But the presence of danger was also testified to by evidence apparent in everyone's experience, such as the death of domestic animals, and this evidence did not confirm the official map of pollution. Animals went on dying in Zone B, officially declared sufficiently 'safe'. The spatial distribution of an epidemic of chloracne (a skin disease due to exposure to dioxin and especially affecting children) did not correspond at all to the distribution of dioxin as indicated in the map.

In fact, Laura Conti suggests, the emphasis laid upon the scientific measurement of dioxin level led to an underestimation of the value of more empirical methods of assessing pollution and to a disregard for the popular experience of danger. Popular initiatives in the decontamination effort were also systematically discouraged. Popular proposals to limit the spreading of pollution were slighted. There was a suggestion, for example, to divert a river before the rainy season, in order to prevent the spreading of dioxin through water, but the proposal was not even considered by the relevant authorities.

Furthermore, according to Laura Conti, the scientists' hope to find a way to test exposure to dioxin in humans actually delayed the implementation of sanitary measures. The anticipation of a scientifically accurate method of detection and measurement of dioxin was detrimental to prompt and effective measures. A fetishistic attitude to scientific measurement was also shared by the local authorities. Months after the drawing of the first map of pollution, new measurements proved that the original map and the three-zones partition were completely untenable, because the distribution of pollution was different and the polluted area far larger than it had been thought at first. The new results were kept secret for months. The reason the authorities gave for this delay was that, given the 'unexpected' character of the new results, they had thought it necessary to repeat the measurements with new samplings, in order to check their correctness. But no new safety measures in relation to the population were thought 'necessary' in the meantime. Measures affecting the health of about thirty thousand people were thus postponed until satisfactory scientific data could be collected. Precaution and common sense had to give way to the cult of scientific certainty.

Examples from Laura Conti's report of a fetishism of scientific measurement among the Seveso authorities could easily be multiplied. This fetishism dramatically contrasted with the popular attitude, which was one of growing incredulity regarding scientific findings in general. Misled and frustrated in their attempt to grasp the real scope of the danger, the people of Seveso finally started to question the existence of dioxin itself. Confronted with contradictory and unintelligible scientific information, they started to view dioxin as 'a big con trick'.



This scepticism found overt and covert manifestations. In October the people evacuated from Zone A reoccupied their homes in a symbolic demonstration which was to be repeated many times, in defiance of the law and of dioxin. They claimed, and, according to Laura Conti, with some justification, that the dioxin level in Zone A was lower than that of the motorway, which had not been closed to traffic. If cars were allowed there, they argued, they ought to be allowed to return to their homes. In fact, the new measurements of dioxin level showed that part of Zone A was 'negative', that is, no dioxin could be detected there with the available instruments. This did not mean, of course, that the place was safe. But 'since a great significance had been attributed to measurements and thresholds, people attached great importance to them; not having understood that with dioxin there is no safety threshold, whenever they saw a negative area on the map of pollution, which included their houses, they demanded immediately to be allowed to return there' (p. 131).

Covert breakings of the safety rules multiplied. The fence enclosing Zone A was systematically pulled down at night, replaced during the day by the local authorities, only to be pulled down again the following night. Disregard for the safety rules was openly displayed. The people mocked the de-contamination workers, who used heavy masks and protections to do the same work that the local women had been doing for months with bare hands and arms. During the night people got into the prohibited Zone to remove property left behind. The responsibility for this can be partly attributed to the local authorities themselves: immediately after the evacuation and enclosure of Zone A, they had allowed 'heads of family' to go back to their houses during the day, under military escort, to take away 'personal possessions', provided that they had been stored in drawers and wardrobes . . . In Zone B, work went on in shops and houses in spite of the prohibition. Most of the safety norms were neglected by the population.

While new alarming reports on the spreading of pollution were published, the relationship between the people of Seveso and the regional authorities got more and more strained. The people of Seveso held the regional authorities responsible for the spreading of dioxin because of their inability to start a serious decontamination programme. They also accused them of using Seveso as a storing place for polluted material from other areas. In turn, the regional authorities, alarmed by the spread of dioxin, sent the army to Seveso to enforce observance of the safety rules. This was tantamount to saying that the people of Seveso were responsible for the spreading of dioxin and had to be treated accordingly, like criminals.

The community reacted wildly and angrily. Episodes occurred which Laura Conti defines as *vendéen*, a term taken from the White Terror in the French Revolution and generally denoting popular counter-revolution and obscurantism. Television operators were violently expelled from Seveso by an angry crowd. The Seveso Council decided not to celebrate the 25th of April, anniversary of Italy's liberation from nazifascism, the most popular of national festivities. 'It was a way to express symbolically their separation from the national community,' Laura Conti comments. 'Seveso withdrew to an angry aloofness, with a feeling of persecution in which every stranger appeared as an enemy (p. 198).'

This reaction can be understood, she suggests, only if we consider that we are dealing with a humiliated community, which desperately clings to its internal solidarity because no support seems to come from the outside. The people of Seveso felt that they had been persecuted rather than helped: had not Seveso been treated like a litter box, the storage place of polluted material from the surrounding area? Had not Seveso been

occupied by the army as a foreign country? Had they not, the victims of pollution, been treated like polluters, like criminals?

In this context Laura Conti tries to understand the reluctance of most people at Seveso to adopt abortion as a precaution against possible foetus malformation due to dioxin. This attitude, she argues, should not simply be attributed to Catholic beliefs; it can be rightly understood within the complex psychological process of adaptation to the new reality after the disaster. The people of Seveso were trying to adapt to the new reality of their town by accepting pollution as a new permanent feature of their life. Pollution, attached to Seveso, as a stigma, had become, in a way, part of the community's self-definition. To accept a malformed baby meant symbolically to accept the consequences of pollution, and therefore to accept the new reality of Seveso itself. It involved an appeal to internal solidarity, well suited to the needs of a community that so badly resented the lack of support from the outside. The Catholic ideas on abortion, implying the malformed foetus' right to life as a human being in its own right, met this deep psychological need to accept the consequences of pollution.

As a politician, Laura Conti is aware that here lies one of the reasons why the Catholic appeal at Seveso found better reception than that of the Communist Party. The Communist strategy at Seveso was based on a call to popular mobilization in the effort to decontaminate the environment. 'Cleaning up' was the Communist formula at Seveso; a formula that, as Laura Conti explains, had a wider political meaning: 'cleaning up, sweeping away dioxin, meant sweeping away many other things, the corruption, inefficiency and inadequacy of the Christian Democrat regime.' The people of Seveso had lost their previous identity, based on work and the accumulation of consumer goods. As a substitute for that, the Communists offered them the identity of a community engaged in the collective effort to clean up their world.

This appeal found very little support among the people of Seveso, as their *vendéen* attitudes showed. Laura Conti tries to explain this fact through the class composition of Seveso society. The economic structure of the town, she argues, was still predominantly precapitalist; most workers were small craftsmen or wage-earners in small factories. If the majority of the Seveso working class had been modern and organised, the *vendéen* attitudes would not have prevailed and the Communist appeal to a literal and political 'cleaning up' would have found a better hearing.

I do not find this analysis convincing. Besides the perplexity that inevitably arises when one reads that the seat of a factory owned by a multinational is still 'precapitalist', I think that her idea of the working class is

an abstract stereotype. She contrasts an abstract craftsmen community, seen as individualistic, competitive, incapable of discipline and of collective effort, with an abstract working class, seen as a compact, disciplined, organic community. I cannot discuss here the implications of this mythical image of a 'modern working class', safeguard of discipline and order in a technological society. I think it exists only in the Communist Party's ideology and it is, in my view, one of its most dangerously conservative aspects.

This stereotype distorts her understanding of the political meaning of the popular behaviour. The people of Seveso, she argues, were unable to individuate their enemy correctly; their hostility to the public authorities, as a matter of fact, was stronger than against the polluter itself, the multinational Givaudan-La Roche. In this fact she sees another instance of the insufficient development of class consciousness at Seveso, again attributing it to a 'precapitalist' economic structure.

Still, this reaction seems to me less 'precapitalist' and irrational than she implies. The people directed their pressure against the public authorities because it would obviously have been less effective to channel it directly against Givaudan-La Roche. They knew that only the state could cope with La Roche and they also knew that the state had left them in the power of the multinational, without any guarantee against exploitation and danger, because of the structural complicity of public powers and private industrial interests. Far from betraying in this a 'precapitalist' political consciousness, the people of Seveso had a correct vision of the structure of power in a highly industrialized society, as founded upon a permanent link between state and private industry.

The Communist Party's strategy is based upon a systematic denial of the link between state and private capitalism; it tends to oppose 'good' public institutions to 'bad' capitalist structures. It is understandable, therefore, that the Communist policy at Seveso was in support of the regional authorities and that the Communists viewed the popular hostility to these authorities as reactionary: *Vendée*. As a member of the Communist Party, Laura Conti shares this view — hence her inability to understand the political meaning of the *vendéen* reactions, in spite of her sympathetic attitude towards the people of Seveso.

She fails to see that the people's apparently 'irrational' behaviour was in fact a reaction to the irrationality of the institutions. Although her description often points to this irrationality, she fails to conceptualize it. It is as if, in spite of what she witnessed herself, she could not help but believe in a core of untainted rationality represented by the public institutions, the state, and, most of all, by science. She takes the fact that at Seveso a

fetishistic attitude to scientific measurement hindered and delayed effective precautionary measures, as a sign that Italian science had been damaged by inefficient government. It is the Christian Democrat regime, she argues, that has undermined the quality of Italian science and that hinders the 'cleaning up', not just of Seveso, but of the whole of Italy. There is, of course, some truth in this, but I want to point out that, in holding a political mismanagement of science as responsible for the episodes she observes, she is protecting science itself from criticism. Relating the discrepancy between the distribution of skin disease and the official map of dioxin level, for example, she writes that the inaccuracy of the map was not a refutation of the scientific knowledge of dioxin. Of course, it was not. Still we ought to consider that the fetishism of measurement and objectivity, the significance attributed to quantitative thresholds, the disregard for popular experience and common sense are all intrinsic aspects of scientific mentality, not only in Italy and not only under a Christian Democrat regime.

Laura Conti's attitude betrays a view of science as a body of true statements and not as a practice. In this view, the disregard for common experience and the consequent loss of information that the practice of science sometimes involves, are obliterated as insignificant; what matters is only the final result: true and useful knowledge of nature. Still, the practical price of this knowledge ought always to be put on the account of scientific practice. The intellectual consequences of scientism have often been pointed out and criticized, but Seveso gives us occasion to reflect upon its practical consequences. The fetishism of scientific measurement has led, in this case, to a disregard for other possible criteria for individuating dangers, especially those arising from common experience, and the pursuit of a scientifically adequate assessment of the level of pollution has delayed precautionary measures. It seems to me that the blame for this cannot be laid upon a misused science, but upon science itself, upon its intrinsic structure of relevances, upon its hierarchy of values, according to which objective and quantitative data matter more than what people experience. This may work very well for certain purposes but it may have negative effects in other cases. Seveso seems to be one such case: instead of speeding up and facilitating the protection of people from danger, the emphasis on scientific measurement and certainty has actually hindered and delayed it.

The popular *vendéen* reaction to the scientific-administrative handling of pollution, described by Laura Conti, should be put within this context. This suggests caution before labelling it as *vendéen*. Popular hostility to scientific knowledge and to coercive measures based upon it (such as, for example, sanitary norms) has always been too easily defined as irrational and too quickly dismissed as reactionary.<sup>1</sup> At least in the case of Seveso we can say that the *vendéen* reaction was brought about by the people's bewilderment and anger when their attempt actively to face pollution was

frustrated and they were left without a clear practical criterion for dealing with a polluted environment. We should remember that these people are not a primitive tribe unexposed to scientific thinking. They have a conception of safety and danger which is scientifically mediated. We ought to ask, perhaps, whether their very absorption of a scientific mentality, with its emphasis on certainty and objectivity, has determined their reaction of angry incredulity when the scientific apparatus has shown its limits. The thresholding and mapping, as Laura Conti recognizes, were motivated primarily by the political need to meet the popular demand for scientific reassurance. No wonder that, in finding out the cheat, the people's reaction was 'to throw out the baby with the bath water'.

There are other ways in which a vulgarized scientific mentality can account for the turn to scepticism by the people of Seveso. Like all of us, they have been taught to think of disease as a consequence of pathogenic organisms which can be isolated and attacked through scientific knowledge and technology. The non specific danger connected with an elusive scientific object like dioxin, whose presence is detected with so much difficulty and whose genetic and pathological consequences are so little known, could not easily be perceived by people used to a conception of disease as produced by a specific cause with specific effects. The pathogenic view of disease, either scientific or vulgarized, may dim our perception of other forms of danger. It may lead to an emphasis on the scientific defeat of some diseases while failing to perceive a general decline of health conditions and of levels of immunity. This, as Laura Conti reminds us, will be the fate of Seveso. The crime of Givaudan-La Roche, she argues, will hide its victims through the statistics of general mortality.

The people of Seveso have sometimes reacted irresponsibly, but the irrationality of their behaviour should be judged in relation to the irrationality implicit in scientific mentality itself. Even their indiscipline against the safety rules can better be understood if we consider the authoritarian and mystifying implications of the scientific conception of health and disease. The pathogenic view of disease implies a strong emphasis on personal hygiene and discipline, and therefore it stresses personal responsibility for health. At Seveso, the safety measures for Zone B, as we have seen, included a strong emphasis on personal hygiene. The *vendéen* trespasses on the prohibited zone and the display of disregard for the safety rules may have been an inarticulate protest against the official emphasis on personal responsibility for health, an emphasis which, in a situation like Seveso's, was obviously a lie.

Laura Conti offers an anecdote. A Seveso married couple go and speak to her, the husband says:

'We've just married; we'd like to have a baby ... We keep our house clean, we take a shower twice a day, we do whatever the Authorities say we should do in Zone B to keep safe ... But

a few days ago she left a window open; some people were harvesting corn, a polluted field, it was a windy day . . . Now there is dioxin in our bed . . . (pp.189-90).'

This image of helpless, blind obedience to official norms should be placed at the side of the *vendéen* actions in order to get a complete picture of a people confronted with the scientific-administrative management of industrial pollution. It is difficult to say which of the two attitudes is more rational. The belief in personal responsibility for health in a situation like Seveso's and the belief that water can wash away the effects of exposure to dioxin seem to me as fully superstitious and blind as pulling down fences or reoccupying homes in the polluted zone. In these last actions, at least, we may trace a meaning as forms of desperate protest.

Any judgement of the behaviour of the people of Seveso ought to be very cautious. I would not follow Laura Conti when she defines it as *vendéen*. It seems to me that her own attentive and sympathetic report shows the inadequacy of a too easy, too quick judgement against this behaviour as irrational and reactionary. Mary Douglas writes:

'Pollution ideas are adaptive and protective. They protect a social system from unpalatable knowledge. They protect a system of ideas from challenge. The ideas rest on classification. Ultimately any form of knowledge depends on principles of classification. But these principles arise out of social experience, sustain a given social pattern and themselves are sustained by it.'<sup>2</sup>

In the comments on Seveso, even those coming from the Left, the unpalatable knowledge seems to be a clear vision of the limits, faults and drawbacks of our science. In the face of pollution its purity is emphasized; in the face of uncertainty, its method appears as the only way to reliable knowledge; in the face of class conflict and cultural conflict, its neutrality and objectivity are stressed. Still, at Seveso, this image has been shaken. Many people there have found it increasingly difficult to maintain an unquestioned belief in science. The struggle for a critical view of science is not fought only within academic circles, between new and old philosophies of science; it is also fought, as the case of Seveso shows, at the level of popular culture and in people's daily lives. The halo of purity surrounding science, sometimes thickened by theoretical dispute, is perhaps most effectively dispelled when many people cannot help but see that science does not always solve the problems of industrial pollution, that its notions of danger and risk are sometimes mystifying, that the cult of certainty and objectivity may actually interfere with prompt and effective precautions against danger.

What we learn from the popular experience at Seveso tends to increase our scepticism regarding the image of science as a neutral and always beneficial instrument of human needs. Scepticism means here a critical attitude, the ability to abandon illusions and to be open to alternatives.

Articulating the dumb protest expressed by the *vendéen* actions at Seveso may well point in this direction rather than towards despair.

## NOTES

1. A particularly nasty and arrogant example of this attitude is given in the article by J. Marmor, V.W. Bernard and P. Ottenberg, 'Psychodynamics of Group Opposition to Health Programs', *American J. Orthopsychiatry*, 30 (1960), which dismisses these forms of opposition as reactionary at a collective level and psychopathological at the individual level. The political meaning of popular resistance to the diffusion of scientific knowledge and its uses for social control has generally been neglected by historians. The only exception I know is the account of prostitutes' resistance to compulsory sanitary examination enforced by the Contagious Diseases Acts in late-Victorian England; see J.R. Walkowicz and D.J. Walkowicz, "'We are not beasts of the fields': Prostitution and the Poor in Plymouth and Southampton under the Contagious Diseases Acts', in M.S. Hartmann and L. Banner (eds.), *Clio's Consciousness Raised: New Perspectives on the History of Women*, N. Y., Harper and Row, 1974.

2. M. Douglas, *Implicit Meanings: Essays in Anthropology*, Routledge, 1975, p.245. Routledge pb, 1978.

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### Science and Society History Workshop — London 23-4 Feb., 1980

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# THREE MILE ISLAND:

## The Ideology of 'Safe Level' as a Material Force

Les Levidow

'People have got to learn to live with the facts of life, and part of the facts of life are fallout.'

— AEC Commissioner Willard Libby, 1955<sup>21</sup>

'When you don't have the facts, you don't have much of anything.'

— Pennsylvania Governor Dick Thornburgh, 1979<sup>3</sup>

Three years after the start of the Seveso catastrophe, we have just now seen a so-called 'near-catastrophe' whose real dangers were mystified in a way similar to those around Seveso. In the Three Mile Island accident, the authorities dismissed the low-level radiation, in effect, as 'part of the facts of life'. Though clearly unprepared — both organisationally and rhetorically — for that accident, they were able nevertheless to appeal to common-sense notions of 'safe level' embedded in the practice of medical X-rays and in scientific/industrial discourse itself.

During those few tense days last April, the authorities attempted to shift popular concern away from the ongoing radiation exposure, and instead towards a possible core meltdown and the massive evacuation thereby required. As with Seveso, despite considerable contamination the authorities recommended only a limited evacuation, which they justified as sufficient by comparing the ongoing leakage to the presumed 'safe level' of the medical X-ray. Not only that, but the safe level miraculously increased during the episode, as the metaphorically benign comparison shifted nearly unnoticed from the dental X-ray (7-20 millirems) to the chest X-ray (30-40 millirems)!

The 'rem', or 'roentgen equivalent in man', is the standard unit of effective radiation absorbed by tissue. It is by no means a physical

measurement of radioactive strength. Rather, the rem was especially devised as a universal equivalent for relating qualitatively different kinds of radioactivity according to the relative biological damage each is presumed to cause. During the Three Mile Island controversy, various writers mentioned such estimates of long-term effects as: 200-250 additional cancers over 30 years per million man-rem<sup>29</sup>; 100 cancer deaths per million people over 25 years per rem<sup>15</sup>; a doubling of the 1 per 2500 rate of spontaneous childhood cancer per rem.<sup>1</sup>

These kinds of statistical estimates presume that a total given amount of radiation absorption will produce the same number of cancers across the population absorbing it, regardless of how little the amount absorbed per person. This assumption is embedded within the form of mathematical calculation; in particular, the 'man-rem' (millirems per person x thousands of persons) means that a large population absorbing a low level of radiation per person can develop as many cancers as a smaller population absorbing a higher level per person. In other words, the 'rem' unit presumes no safe level whatsoever. Or, as George Wald argued during the Harrisburg crisis, 'Every dose of radiation is an overdose.'<sup>28</sup>

Yet the authorities easily evaded the cancerous meaning of their radiation measurements by comparing them to yet other measurements — medical X-rays and background radiation.<sup>(A)</sup> This fetishism of 'millirems per person', appealing to a negative 'possessive individualist' attitude towards possible damage, helped the state to perform its crucial tasks: to contain the immediate threat of popular disturbance, especially a break in production, as well as general distrust in the government itself; and to protect the nuclear industry from subsequent attack for the health hazards inherent in it. Thus the industry was able to congratulate itself afterwards for having prevented a core meltdown; in this way the state was able to protect capital's interest in nuclear power.

### The 'Dental X-Rays' Begin

Let's retell the story of Three Mile Island from that perspective. On the first day (Wednesday 28 March) Metropolitan Edison (Met Ed), which operates the plant, boldly claimed, 'There have been no recordings of any significant levels of radiation, and none are expected outside the plant.'<sup>28</sup> Meanwhile the Nuclear Regulatory Commission in Washington admitted that 'low levels of radiation' had been measured up to a mile from the plant but claimed that the amounts were far below what is considered *immediately* dangerous to health.<sup>10</sup> 'Immediately' meant simply that there was no threat of radiation sickness or worse from the putative 3-7 millirem/hour exposure rate.

The authorities concealed the long-term cancerous effects by benignly representing that rate as 'no more than you get in a dental X-ray'.<sup>28</sup> In so doing, they were comparing incommensurables: a continuous rate with a finite quantity, and visceral organs with teeth. Or, as one UK writer put it in retrospect:

'The people of Harrisburg, it seems, were getting a whole body dose, including a dose to the interior of the lungs, equivalent to between 3 and 10 dental X-rays a day.'<sup>29</sup>

Even that critical commentary, effectively calling the authorities liars, did so within the authorities' own fetishized terms of reference; it disputed *quantitatively* the government's ultimately meaningless comparison of two different processes: infrequent 30-second exposures to a section of the jaw, and an 'equivalent' total concentration of exposure to more cancer-prone parts of the body spread over several days.

The official claims were not merely quantitatively false in content but also ideological in form, representing the (largely hidden) organic damage as some finite, familiar thing popularly associated with good health. Accordingly, to the many inquiries from pregnant women, the Governor's office initially replied that they saw 'no need to consider evacuation at this time'.<sup>11</sup>

Then on the third day (Friday), Metropolitan Edison — without telling anyone in advance — attempted to relieve the pressure problem by deliberately releasing a large plume of radioactive gas<sup>7</sup>, noticeably increasing the measured radioactivity nearby. The initial reaction of the Nuclear Regulatory Commission (NRC) to the radioactive plume was revealed only 2 weeks later, when tape recordings of their internal discussions were made public. Early that Friday morning (9:15 am), NRC officials advised the Pennsylvania state police to evacuate *everyone* within a 5 mile radius of the plant. An hour later the commissioners changed their minds, supposedly because some people might get even larger doses of the radiation if they went elsewhere!<sup>20</sup>

Harold Denton, the NRC's chief scientist and Carter's on-the-spot agent, acknowledged in the NRC's private discussions that after Met Ed opened the valves there was a 17 millirad per hour contamination rate as far as 5 miles away from the plant.<sup>(B)</sup> 'I think the important thing for evacuation to get ahead of the plume is to get a start rather than sitting here waiting to die.' Yet he announced no such thing publicly. Another commissioner objected, 'I don't have a reason for not moving people. I don't know what you are protecting by not moving people.'<sup>4</sup> The NRC Chairman, Dr. Joseph Hendrie, told the government at one point that the off-site radiation was below the 'trigger-levels' for evacuation set by the

Environmental Protection Agency (EPA), but added, 'On the other hand, it certainly is a pretty husky dose rate.'<sup>16</sup> As one writer later described that dose rate:

'The initial exposure rates, the news media reported, were about 15-30 milliroentgens per hour five to ten miles downwind. To compare this to the 20-milliroentgens dose of a single dental X-ray is misleading because only a small portion of the jaw is exposed for less than a second. In Pennsylvania, the entire human body was exposed to this dose hour after hour.'<sup>18</sup>

### **Mass Evacuation Begins**

John G. Herbein, Vice-President for Power Generation at Met Ed, continued nevertheless to make the dental X-ray comparison to allay fears. But several thousand schoolchildren were evacuated from the plant area early in the day, and the Governor finally announced, 'I am advising those who may be particularly susceptible to the effects of radiation — that is, pregnant women and pre-school age children — to leave the area within a five-mile radius until further notice.'<sup>12</sup> He also ordered 23 schools closed in and near Middletown, and urged people living within 10 miles of the plant to remain indoors.<sup>7</sup> That radius conveniently excluded the entire city of Harrisburg.

The response of people nearby was not so tidy. For example, by that evening 150 pregnant women and young children were staying at a shelter in Hershey.<sup>12</sup> But over the succeeding days there were only a fraction of the number of pregnant women expected:

'One theory that accounts for the small percentage is that the women, knowing their susceptibility to radiation, kept driving right past the Hershey Sports Arena. It is located only 14 miles from the damaged nuclear plant.'<sup>23</sup>

In addition, although ten thousand residents had evacuated themselves between Wednesday and Thursday, now the Governor's warning unintentionally set off a truly mass exodus including an estimated 40% (14,000) of the population living within 5 miles of the plant<sup>26</sup>, and up to a total of 100,000 of those living within a 20-mile radius, or nearly one-tenth of the Harrisburg metropolitan population.

With no noticeable effect of the radioactivity as such, it struck many people as much less tangible than some immediate social consequences. According to a gun shop owner whose sales jumped during the crisis period:

'A lot of people didn't understand the threat because they couldn't see it. So I think they transferred their fears into more practical concerns they could deal with, like having their homes looted if they had to evacuate, or running out of food if all the stores closed. It would

have been different if the radiation had blown over us in a red cloud which people could have seen, rather than this invisible gas. Then they would have panicked.<sup>9</sup>

As the interviewer explained, instead of leaving the area altogether many residents not only bought guns and bullets and canned food but also filled the petrol tanks of their cars. Of those who did evacuate their families, many later returned to the area a little embarrassed to confront neighbours who stayed behind and 'didn't feel a thing'.<sup>5</sup>

Throughout the Three Mile Island crisis, government officials avoided speaking directly to the popular ambivalence towards the supposedly 'safe level'. They concentrated on publicizing their own efforts at averting a possible core meltdown and planning a massive 'precautionary evacuation' in case an imminent meltdown seemed to warrant one. They tried to project fears about the existing leakage onto doomsday visions which they in turn described with various euphemisms; perhaps the oddest was, 'in the event of an event . . .', almost a parody on itself — and as if an 'event' had not already occurred!

Of course, the official speculation assumed that the Governor's modest evacuation recommendations of March 30th remained adequate to the present danger. Certainly that limited evacuation was far less an inconvenience — indeed, threat — to the government than a *truly* 'precautionary evacuation' undertaken to avoid increased cancer deaths 30 years later resulting from multiple 'dental X-rays'. As President Carter was to put his praise of Governor Thornburgh:

'... because of the trust of the American people in him, and particularly those who live in this region, potential panic and disturbance has been minimized.'<sup>13</sup>

### **Return to 'Normality'**

As a meltdown came to seem less likely and the radiation leakage seemed to decrease, by April 2nd most people were returning to their homes, except those living nearest the plant. Remaining doubts found a reply from Harold Denton, who claimed that the maximum radiation exposure anyone had so far received outside the plant was about 100 millirems, which he likened to 'a *whole-body* X-ray' (emphasis mine).<sup>13</sup>

Denton also attempted to minimize the danger from the iodine-131 emissions by favourably comparing the amount now measured in milk with that found during the Chinese nuclear test, which at the time was deemed well below the Food & Drug Administration's so-called 'upper limit for tolerance'.<sup>14</sup> Like his X-ray comparison, this iodine claim entailed a bit of circular reasoning, presuming some 'safe level' for a

radioactive isotope of an element readily absorbed and accumulated in the thyroid gland. Nevertheless Safeway stores temporarily stopped buying milk from the area's dairy distributors.<sup>14</sup> And there was official silence on the possible emission of other radioactive substances, especially those with a half-life\* much longer than that of iodine.

Popular doubt centred on the apparent inconsistency between the government's reassurances, on the one hand, and the limited evacuation it had ordered, on the other. People were asking such questions as:

'If, as the authorities insist, radiation to people in the area was not more than they would have received from a dental X-ray, why were pregnant women and children under five evacuated from a five-mile radius of the reactor? And if there is no longer any escape of radioactive gas, why can no date be given for their return? Are those who remained really safe?'<sup>15</sup>

These doubts seemed to be relieved only when on April 9th the Governor announced the end of the reactor core crisis and declared the affected area now safe for pregnant women and young children to return.<sup>24</sup> The next day, scores of people received free radiation tests and were told the computer checks confirmed that the radiation levels in their bodies were 'normal'<sup>22</sup> — as if such a measurement could say anything about organic damage already done by the earlier exposure.

Harold Denton again attempted to minimize the overall damage done, this time by equating the maximum possible radiation exposure so far (80 millirems) with the amount from two *chest* X-rays (emphasis mine).<sup>26</sup> Joseph Califano, Secretary of the Department of Health, Education and Welfare, parroted Denton's claim and went on to compare the estimated 80 millirems favourably to the 200 millirems absorbed annually by the 'average American', as yet another implicit 'safe level'. He went as far as to testify before a Senate subcommittee that he expected no additional cancer deaths among the population within 50 miles of the plant.<sup>27</sup> At most he acknowledged that 'great uncertainties still remain about the relationship between cancer and low-level radiation'.<sup>28</sup>

### **'Just a Couple of Chest X-Rays . . .'**

Discrepancies abounded. Other sources claimed that the exposure was equivalent not to 2 but as many as 4-5 chest X-rays<sup>24</sup>; or that 50 additional cancer deaths could be expected<sup>15</sup>; or that foetal radiation exposure in particular may increase the spontaneous rate of childhood cancer.<sup>2</sup> And

\* 'Half-life' is the time required for a radioactive isotope's amount of ionizing radiation to diminish by half: 8 days for iodine.

those sources (as well as others<sup>17 18</sup>) explicitly challenged the notion of a 'safe level' below which there is no significant damage. Yet the authorities were still able to allay fears by comparing the additional exposure rates to radiation absorbed from medical X-rays and natural background — as if these 'normal' levels were themselves perfectly safe, and small increments no great problem!



A case in point of how the 'safe level'-syndrome continues to plague even its sceptics: for the area surrounding a plutonium processing plant operating for decades near Denver, the officially predicted damage amounted to only one cancer death more than the number normally expected for the 1.6 million people living nearby. Yet a study published during the Harrisburg crisis discovered 501 additional cases of cancer there. At the same time, having *statistically* demolished whatever 'safe level' the official prediction had presupposed, the study nevertheless referred to one particular emission there as having reached 19,000 times the amount regarded as the 'safe maximum' by the US Department of Energy!<sup>19</sup> Such comparisons continue to be made even by those whose criticisms make them meaningless; 'safe level' is not defeated as a material force by counter-facts alone.<sup>(C)</sup>

And after all this, what was capital's assessment of the Harrisburg crisis?

'To put the best face on things, this "worst" accident in the industry's history caused no measurable health or property damage beyond the plant itself.' — *New York Times* editorial, 12 April 1979.<sup>22</sup>

'We didn't injure anybody, we didn't seriously contaminate anybody and we certainly didn't kill anybody.' — John Herbein, Vice-Chairman of Metropolitan Edison<sup>18</sup>

'That incident will prove in the long run that nuclear plants are safe. Everything went wrong that the anti-nuclear people said would inevitably result in a catastrophe. There was no catastrophe — just a couple of chest X-rays. There was no meltdown. That's significant.' — Peter J. McTague, President of Green Mountain Power Corp. of Burlington, Vermont.<sup>20</sup>

'... potential panic and disturbance has been minimized.' — President Carter.<sup>3</sup>

So this is how the state minimized disruption of the normal relations of production, in both the short and long term. Through the scientific terms of reference it invoked, the state was able to represent the low-level radiation exposure as 'part of the facts of life'.<sup>(D)</sup> This not-uncommon feature of the nuclear industry — an inherent contradiction between the economic profitability and public acceptance of nuclear power — thus became a merely contingent conflict: the monitoring of 'safe level'. In this way the state could appear to be protecting people from the excesses of the industry while really protecting capital's interests in nuclear power: a sophisticated exercise in managing the contradictions inherent in the nuclear industry.

Yet the question for us still remains: Even 'knowing better', what can we do about it? How can we make the *de-fetishization* of 'safe-level' a material force against capital's control?

## Notes

A. How did the authorities so easily evade clarifying the origin and meaning of their own radiation measurements, by comparing them to other quantities rather than to cancer rates? Namely, because the scientific terminology itself contains the potentiality for mystifying the emissions in terms of medical X-rays and natural background radiation. With the 'rem', a definite social relation — cancer causes — gets converted into a universal equivalent which can then take on a life of its own in scientific/political discourse, so that socially constructed X-rays are then taken as 'natural' standards of measurement.

So the non-neutrality of that discourse extended far beyond innocuous-sounding quantitative comparisons, all the way to the basic unit of measurement itself. To measure and speak in 'rems' was to accept — however unwittingly or even unintentionally — implicit social assumptions as to what is 'natural', what counts as damage, and how it can be predicted. The apparent technical neutrality of the 'rem' helped the authorities to ideologize medical X-rays and natural background radiation as unproblematic bases of comparison, as literally part of the facts of life.

As Marx said in the *Grundrisse*, taking the example of labour:

'... even the most abstract categories — despite their validity for all epochs (precisely because of their abstractness) — are nevertheless, in the specific character of this abstraction, themselves likewise a product of historic relations, and possess their full validity only for and within these relations (Penguin, 1973, pp.104-5).'

How do we find a way of using radiological terms so as to clarify rather than obscure their historical specificity and social assumptions?

B. The rad, or Radiation Absorbed Dose, is a physical quantity of radiation corresponding to the amount that will cause 1kg of material to absorb 0.01 joules of energy — very little energy compared to its equivalent in heat, but extremely harmful because of its ionising form. The resulting amount of 'rems' is computed by multiplying the amount in rads by a 'quality factor' ( $\geq 1$ ) expressing the relative biological harmfulness of the different kinds of radiation involved. The factor is 1 for beta- or X-rays, and 10 for alpha-rays or neutrons.<sup>6</sup> So the effective radiation absorption by tissue was *at least* 17 millirems per hour.

C. Notice the stubborn persistence of 'safe level' in the definitive official UK study, the Flowers Report<sup>6</sup>, which apparently giveth with one hand and taketh away with the other:

'It is assumed in radiological practice and in the control of occupational radiation hazards that there is no threshold dose needed to induce cancer and that the effect of radiation is directly proportional to the dose down to the lowest dose and dose-rates(p.19).'  
'Although it would be foolish to suppose that all the standards are so well established that further research work will not require their amendment, there does appear to be more agreement over what can be tolerated than there is for other pollutants(p.83).'

D. Potential sources of radiation exposure include not only the power plants but also the waste disposal — over 90% of which comes from military uses rather than from commercial reactors (*National Geographic* April 1979, pp.477, 480). See for example the study by Thomas Mancuso and others (*Health Physics* Vol. 33, p.369)<sup>17</sup> on the enormous cancerous effects on workers at the military reprocessing plant at Hanford, Washington state — a vast complex popularly referred to as 'the world's biggest nuclear graveyard'.<sup>25</sup>

The alleged safety of even 'normal' radiation exposure was challenged in the briefing document prepared by the Drake's Farm collective on behalf of the plaintiff in the 'Honicker vs. Hendrie' court case for damages against the NRC. For an account of the court proceedings, see the collective's book edited by Matthew McClure, *Shut-Down*, published by Paul Mandelstein, 1979. (Copies available for \$5, or \$3 each for five or more copies, from: The Farm, Box 156, Summertown, Tennessee 38483.)

Just as RSJ9 went to press, an anti-'safe level' position was endorsed by a majority on the US National Academy of Sciences committee investigating the Biological Effects of Ionising Radiation (BEIR), as reported (e.g.) by David Dickson, 'US Academy Denies Threshold for Radiation Damage', *Nature* 279 (10 May 1979), 90-1.

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- Karl Z. Morgan, 'How Dangerous Is Low-Level Radiation?', *New Scientist* (5 April 1979), 18-21. (ref.17)
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- Anthony Tucker, 'How Today's Radiation Level Affects Tomorrow's Cancer Toll', *The Guardian* (2 April 1979), 13. (ref.29)
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## STRUGGLING AGAINST CONSUMPTION OF/BY CHIPS AND BUGS

Science and Technology are increasingly important to capital in ways that impinge on all of us. This is dramatically true of microprocessors and of genetic engineering. *New Scientist* and *Nature* cover developments and establishment debates but miss out class struggle and wider issues. *RSJ* will publish on these issues, beginning with *RSJ* 10. For now, here are some useful recent publications.

- Counter Information Service, *The New Technology*, CIS Report pamphlet, 1979. Pp.40. 75p. (9 Poland St., London W1V 3DG).
- Women's Voice Word Processor Pamphlet, *Job Massacre at the Office*, 1979. Pp.22. 25p. (PO Box 82, London E2 8DN).
- Chris Harman, *Is a Machine After Your Job? New Technology and the Struggle for Socialism*, Socialist Workers' Party pamphlet, 1979. Pp.32. 40p. (SWP Training Dept., PO Box 82, London E2).
- Changing Role of S.E. Asian Women*, Special joint issue of *Southeast Asia Chronicle* (no.66) and *Pacific Research* (nos. 5-6), 1979. Pp.28. \$1.00 (PO Box 4000D, Berkeley, CA 94704, USA) — about the 1,000,000 women who work for US corporations, no union, eye damage after 3-4 years and sacked; also about subsequent job opportunities as Philippine hospitality girls.
- The Economist, *Chips and Bugs — Tiny Technologies with a Big Future*, pamphlet, 1979. Pp.20. £2.50(!) — articles from *The Economist* (25 St. James's St., London SW1A 1HG).
- APEX Word Processing Working Party, *Office Technology: The Trade Union Response*, APEX pamphlet, 1979. Pp.68. 40p. (22 Worpole Rd., London SW19 4DF).
- ASTMS, *Technological Change and Collective Bargaining*, ASTMS Discussion Document, 1979. n.p. (26 Jamestown Rd., London NW1 7DT).
- Ernest Braun and Stuart MacDonald, *Revolution in Miniature: The History and Impact of Semiconductor Electronics*, Cambridge University Press, 1978.
- Iann Barron and Ray Curnow, *The Future of Microelectronics: Forecasting the Effects of Information Technology*, Frances Pinter, 1979; soon to be Open Univ. pb.
- Clive Jenkins and Barrie Sherman, *The Collapse of Work*, Eyre Methuen pb, 1979.
- (We recommend adding 20p for postage.)

# HARD SPHERES

**Daniel Schiff**

Opening lecture of the first year physics course . . . one hundred and fifty pairs of unknown eyes . . . one minute to the off . . . a stomach cramp. 'What the hell am I doing here?' I take the plunge:

'The characteristic of Science is that experiment is the test of all knowledge. Experiment is the sole arbiter of truth . . .'

These words, taken from the very best of sources (Feynman Physics Lectures) act like magic on my inner discomfort.

' . . . Physics: imagine that some cataclysm has swept away all scientific knowledge. Only one sentence can be handed down to posterity; what do you think would convey the maximum of information in the minimum of words?'

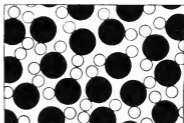
Hands go up: 'Force equals mass times acceleration — No.' 'The Conservation of Energy — No.' 'To be or not to be,' suggests a wit.

'I think the right sentence would be something like: EVERYTHING IS COMPOSED OF ATOMS, LITTLE SPHERES IN CONTINUAL MOVEMENT WHICH ATTRACT ONE ANOTHER, BUT ON BEING BROUGHT TOO CLOSE TOGETHER ARE AS MUTUALLY IMPENETRABLE AS BILLIARD BALLS.'

This is something of a let down; nothing here to get worked up about, seems to be their reaction.

'Consider for example a drop of water. If you looked at it with a million times magnifying microscope, what would you see?'

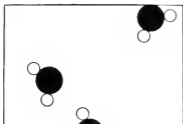
I draw a picture



WATER MAGNIFIED ONE BILLION TIMES

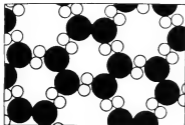
‘Teddy bears!’ observes the joker from somewhere behind me.

‘The big spheres are oxygen atoms, the little ones hydrogen atoms.’



STEAM

I glance at the clock — a mere three quarters of an hour to go. I heat up the drop and the atoms move around more and more quickly until they separate: water vapour.

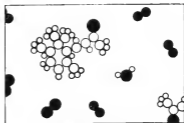


ICE

I freeze the drop and the atoms slow down and arrange themselves in a regular pattern to form ice.

And then, to show that these atoms are no strangers to the world of human experience:

‘If you go into a wood where there are violets the air is scented. What is this perfume? It is a certain variety of molecule, that is to say a certain configuration of atoms in the air above the violets. These molecules move randomly in the air, pushed hither and thither until, by some chance, they reach your nostrils.’



ODOR OF VIOLETS

In this manner the hour passes; I've done liquids, gases, solids and the scent of violets. Here endeth the first lecture.

All right, I know; instead of stressing the invisible simplicity hidden beneath the visible complexity, I should have taken *their* knowledge as my starting point. I *do* know that in China the physics course starts with making a motor for a moped and with building a radio set and I *do* remember a friend telling me how in Allende's Chile he taught physics to workers in the factory using their machines. But I wouldn't be able to manage that: I am much happier with 'thought experiments' than with experiments done for real. I'm by no means the only one though: I remember some Maoist students who, in the aftermath of '68, reproached me with teaching quantum mechanics in too abstract a manner, at too great a distance from the concerns of the masses. I should have been serving the interests of the people . . . today those same students are doing the most esoteric theoretical physics in the most elitist of labs!

On the couch, for years I talked about almost everything that was going on in my head, but I never spoke about physics. Just occasionally I would mention my panic when on a previous day a student had asked me a question to which I should have known the answer. At most, I would speak of the invisible ties between myself and other researchers, the feelings of complicity, rivalry and friendship which, beneath all the equations and formulae, I sensed between us. Say it with figures. I never spoke of the hard core, the subject itself: I left the molecules in the waiting room. Then one day, I decided to let them in. 'Once I start thinking about all the molecules which, at this very moment, are swanning around this room right, left and centre at fantastic speeds (three hundred metres per second), then I can't ignore them however hard I try. I can't not be interested in physics, it does something to me . . . We can't see the molecules moving and we can't feel them either, but they are there all the same, they *are* there.'

Silence; the molecules tack noiselessly to and fro.

Then he says: 'Say if they're not really there?' 'But . . . but they are there, and that's the whole point they *really are* there.' He can't understand. I remain silent for the rest of the session leaving him cheerfully indifferent to the molecules which even so are still flitting around the room.

This suddenly puts me in mind of another anecdote which I read in Feynman:

'One of the most remarkable discoveries concerns the source of the energy of stars; the reason why stars shine. One of the men who discovered this in the thirties was out walking with his girl-friend on the same evening that he had realized that it must be *nuclear* reactions which enable the stars to keep on shining. She said to him, "Aren't they beautiful, all those stars," and he replied, "Yes, and at this very moment I am the only man on earth who knows *why* they shine."'

'This made her laugh; she was not in the least bit impressed by being out with the only man who, at that instant, knew why stars shine.'

'Well,' remarks Feynman, 'it's sad to be alone but that is the way it is, in this world.'

Would you like me to tell you what makes the stars shine, what makes the sky blue and why the sunset is red? No thank you. (*What is it women want?*) Talk to me about me.

A night of no stars . . . thirty eight years old . . . I thought I'd done with this kind of doubt and despair. How easily I am disturbed — it takes not more than a glance . . . how frail I am. We are both in tears. But what does she want? What do I want her to want? How is life to be lived?

I get in to the lab the next morning. 'You know, I got it wrong yesterday for argon; I've redone the calculation assuming spheres of smaller diameter and I think that that will give us the experimental value spot on . . . Look —.' He goes to the blackboard and starts slowly to set out the white symbols. 'Good, now this term goes out because of symmetry, OK?' Little by little, his untroubled mood communicates itself. We have all the time in the world, thirty eight years, seventy six years . . . '— that leaves the integral for three hard spheres, but we can calculate that by the method of . . .' I feel good again, lean back in my chair, feet on the desk, pencil in mouth: 'No, but what we ought to do now is to add in the quantum correction and then we can also explain the value for neon.' We go next door to talk it through with the neon experts. Yes, that's doable; they'll calculate the quantum correction for us. Quite soon, or tomorrow, we will know whether that explains the experimental differences. Otherwise we'll have to think up another explanation. We are as impatient as children.

Does the desire for knowledge originate in the sexual curiosity of the child? Perhaps, though I find curiosity a rather detached, unemotional expression, conveying little of the mystery and anguish which exists and not only for the child. Desire for knowledge? As far as I know(!), I wasn't particularly interested in understanding how neon atoms interact. But what is there to think about when you aren't thinking of anything in particular? Dozing off, at the moment of waking or during a bad film,

images of hard spheres pass before my eyes. And it is often that my own personal half-conscious fragments of thoughts, plans, speculations come to condense around or are sublimated into these little spheres: '... integral for three hard spheres ... case where one is distant from the two others ... factors separable ... must check.'

The pleasure of things clicking into place actually becomes addictive. One is continually drawn back to that game of going to-and-fro: if it doesn't check with experiment then find another hypothesis; another question, another answer; build up tension, achieve release; pose the problem, solve it; begin again ...

In the end neon atoms don't matter very much; the important thing is being able to frame 'proper' questions: questions guaranteed to have answers. Whereas the questions that live in me and with me ...

Comprehensive insurance policy: we have shown that the integral for three hard sphere accounts/does not account for the experimental differences between argon and neon. It gives me protection, very personal cover: my calculations square with nature. We are within the error bars. We have calculated the uncertainty ...

'But what of the social? How can you talk about physics with no reference to hierarchy, institution, social recognition, division of labour, capitalism! You say nothing about the socio-historic determinants, nothing of the ideological role of this physics in this society: what do you think you're paid for? You know well enough that not bringing in politics is a kind of politics. Making a separation between your immutable ahistoric molecules on the one hand, and on the other, yourself, whom you imagine to be equally irreducible, singular ineffable ... you with all your doubts and sexual hangups, that's socially determined if anything is! School, dominant ideology, etc ...'

A madman, who thought he was a mouse is cured and leaves hospital only to come running back: 'I bumped into a cat – but you know you're not a mouse – yes, but does the cat know it yet?'

Yes, my thoughts are socialized:– but what about the molecules?

How does this society work, why does it pay me, put ideas in my head? How, conversely, do I transform it albeit infinitesimally? I don't know, it's a total mystery; it's opaque. But to know that one can derive this or that visible property of matter from some interaction or another between these minute particles, that's reassuring; transparent. Matter is nothing more than an assembly of molecules.

'The stars are composed of the same atoms as is the earth. I normally base a lecture on this sort of proposition. Science, poets say, tarnishes somewhat the beautiful image of the stars by describing them as simple agglomerations of gas atoms. But nothing can be "simple". I too can respond to the sight of the stars in a desert night. But do I see less or more? The vast celestial vault exalts my imagination. Fixed on this moving pageant my own little eye is able to capture light a million years old. A huge structure of which I am part — perhaps even my own substance is the product of some forgotten star like one of those shining out there now. Or if I look at them with the great eye of Palomar, they all seem to rush away from a common point where once perhaps they were all gathered together. What is the structure, the meaning, the purpose? The mystery is not devalued by a little knowledge, for the truth is more marvellous than artists have imagined in the past! Why don't our contemporary poets write about this? What sort of poets are they who can talk about Jupiter when he took human shape but fall silent when we discover that Jupiter is a huge rotating sphere of methane and ammonia?'

It makes no difference having lost faith to the point of no longer doing physics. I still experience a tension: how is it that I can find Feynman to be both true and at the same time meaningless?

It is in trying to finish off this paper that I find myself most regretting that it is not a paper on Physics. It would be so nice to be able to draw 'conclusions', e.g., the reader has learned the following from what goes before.

'It has been shown that recent measurements for argon are consistent, to an accuracy of about 10%, with a hard sphere model.' Modest, but solid: after the article things are not precisely as they were before. And now a pear for dessert: 'the question of whether quantum corrections involving three-hard-sphere integrals permit one to explain the experimental results in the case of neon will form the subject of a forthcoming paper.' Whereas what has this piece I'm writing now taught anybody? What support do I have for my words? None. They don't hold together; I have to legitimate them myself. There is no 'scientific' framework: '... a comparative study of the libidinal investments of men/women, physicists/mathematicians, theoreticians/experimentalists has shown that ...'

It is not backed by 'militants': '... a new relationship to knowledge will only be possible when the working class, oppressed minorities, women ...'

So, if I refuse both the 'scientific' and the 'militant' styles, what justification is left for my writing a paper? I have simply been playing with words to make myself appear hard, like a hard sphere.

But that may leave more room for other people. I spin a web of words in order to try and be less alone.

*Translated from the French by Stephanie Barnett and Mike Barnett*

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# REVIEWS

## CONNECTIONS

**Alfred Sohn-Rethel, *Intellectual and Manual Labour, A Critique of Epistemology*.**

**Macmillan pb, 1978, Pp.xiv + 213, £3.95.**

**Reviewed by Chris Knee**

*Live in fragments no longer. Only connect and the beast and the monk, robbed of the isolation that is life to either, will die (E.M. Forster, Howards End).*

My interest in Sohn-Rethel's work developed while working in the sociology of mathematics. I originally saw mathematics as a 'hard-case' for the sociology of knowledge. In other words, I saw no reason to exclude mathematics from the study of the social origins of, and interests served by, bodies of knowledge. More positively, I saw a necessity to deal with mathematics precisely because of the power it exercises on our thinking and working in all areas of life. It seemed to me that mathematics, like science in some ways, was too easily accepted as unbiased, timeless, universal, neutral knowledge, and therefore, like other 'neutral' knowledge, had a politics which needed to be clarified.

However I gradually discovered why, for different reasons, in different cases, mathematics was not on the agenda of the sociology of knowledge. Mannheim excluded mathematics on the grounds that it was a special case and David Bloor's pioneering attempt to overcome this relativism suffers from his attempt to make the sociology of knowledge 'scientific'. The main approach from within Marxism was one which explained developments in thought simplistically as directly functional to the needs of production. A more subtle approach illustrated by 'phenomenological Marxism' seemed unable to avoid charges of idealism. I was searching for a historical materialism of mathematics, the model of rational thought. It was in this context that I came upon the work of Alfred Sohn-Rethel who claimed to show that 'the logic of scientific thought originates in social history' (Sohn-Rethel, 1975, p.73). This book is not easy to read, all the more so as Sohn-Rethel's ideas are compressed into a curiously short volume given that it is dealing with 2000 years of history. Also some of the concepts need a new language to express them.

Alfred Sohn-Rethel's *Intellectual and Manual Labour* is the result of an investigation that began over 50 years before its publication in 1970. It is the product of a remarkable 'thought-biography' beginning during the First World War alongside Ernst Bloch, Walter Benjamin, Max Horkheimer and Theodor W. Adorno, in the political atmosphere of the failure of the German proletarian revolution<sup>1</sup>, and leading, through a reading of Marx, 'with an effort of concentration bordering on madness' (p.xiii), to the idea which was to become an *idée fixe* to him. The enquiry into the relation of base to superstructure, 'the secret identity of commodity form and thought form' (p.xiii), was the starting point of this monumental work. The originality of his idea, lying in its fundamental challenge to a deeply embedded aspect of bourgeois thought, the separation of intellectual from manual labour, was the reason for Sohn-Rethel's being condemned to live the life of an outsider. (The first manuscript of this book was finished in 1951 but was consistently turned down by publishers for being either too unorthodox or too militantly Marxist.) Sohn-Rethel was rejected by all but a few kindred spirits, among them Adorno and George Thomson<sup>2</sup>. *Intellectual and Manual Labour* is undoubtedly an important book not only because of the scope of its enterprise but also because of its political relevance to Marxists involved with science — a relevance due to its limitations as well as to its many and undeniable advances.

I will attempt to underline that relevance through an account of the principal concerns of the book which revolve around Sohn-Rethel's concept of 'real abstraction'. Marx's view that 'It is not the consciousness of men that determines their being, but, on the contrary, their social being that determines their consciousness' (quoted on page 5) leads Sohn-Rethel to scrutinize even those areas of consciousness that appear neutral and unbiased — such as the very content of mathematical knowledge — in terms of their social determinants. I will describe first how Sohn-Rethel investigates different key moments in history and attempts to connect economic and intellectual production in each of these periods. I will then develop my reservations about the book by suggesting that its limitations are due to the way in which he chooses to criticize Kant's philosophical epistemology. His critique of the Kantian model does not succeed in freeing itself from the restrictions of that model but too often simply adopts opposite concepts (for example by replacing Kant's 'transcendental synthesis *a priori*' with his own 'social synthesis'). Yet even these limitations have the value of indicating what could constitute a real alternative in a Marxist critique of science.

'A historical materialist explanation of the origins of scientific thought and its development is one of the areas by which Marxist theory should be extended (p.3).' Sohn-Rethel conducts this explanation of origins on the

basis of what he calls the social synthesis: 'the network of relations by which society forms a coherent whole' (p.4). It is the social synthesis that determines consciousness, or rather the conceptual foundations of the cognitive faculty. The relation between thought form and commodity form has long been an important issue for all those concerned to avoid vulgar Marxism, for example, the crude simplifications of reflection theory (the theory explaining science as a reflection of its object in nature). Although we may deny their separation in theory, we have no practical way of bridging the gap between thought and commodity.

Paul Piccone, the editor of the journal *Telos*, situating himself in a debate centering on the works of Gramsci, Lukács, Sartre, Marcuse, seems to be concerned with precisely this problem: 'The process of concept production should not be seen as at all different from the process of commodity production (Piccone, p.17).' However, this complete identity, although an attempt to resolve the problems inherent in a mere synthesis of two separate entities, allows no vestige of autonomy to either. Thus, there seems to be no middle way between relating a separate thought form to a commodity form, and a complete identity of the two. Sohn-Rethel points out that Marx paved the way for such a new approach by being the first to express that abstraction originates in commodity exchange and is not the exclusive property of the mind.

'The form of commodity is abstract and abstractness governs its whole orbit (p.19).' What is more, this abstraction is a product of human actions, not of the human mind. Therefore, what Sohn-Rethel is defining is a 'real abstraction'. It is *real* in that it results from spatio-temporal activity; it is *abstract* in that the act of commodity exchange implies its separation from use and appears as an objective social law. He describes commodity exchange as the original source of abstraction which is, in turn, the basis of the cognitive faculty of conceptual thinking, and argues that real abstraction engenders the ideal abstraction which is basic to modern science.

Sohn-Rethel's thesis is that abstraction was born in ancient Greece at the time of the first appearance of coinage, the material manifestation of the abstraction of commodity exchange. All class societies based on commodity production are characterized by this mode of thinking. This mode is rooted in the ideal abstraction, itself a conversion from the real abstraction of exchange, and results in the 'independent intellect'.

The title of the book is now justified: the winding path from the exchange abstraction of coinage has reached a first clearing, 'autonomous' intellectual labour. At the same time as coinage was developed, its intellectual counterpart, Greek geometry, appeared. In Greek geometry, 'the

manual operation became subordinated to an act of pure thought' and 'the conceptual content was independent from any practical task'. What is more, 'this could result only through the generalization intrinsic in the monetary commensuration of commodity values promoted by coinage' (p.102). Euclid's *Elements of Geometry*, still used as a textbook in Britain well into the 19th century, is the finest example of these developments.

Sohn-Rethel describes the eventual death (or suicide as it has been described) of the ancient Greek economy as due to the system's salient feature while it was flourishing, 'the social category of value as money and as capital... failed to communicate its social character to labour' (p.104). Thus, just as labour was not human labour in the slave economy and the transition to feudalism, it does not become human labour until the renewed cooperation of labour in production takes place with merchant-capitalist developments. Most histories of mathematics seem to describe this middle period as a mathematical desert, at least in Europe, and mention it only in relation to translation of Greek mathematics or to Hindu and Arab mathematics.

In the subsequent transition from artisanry to science, the state of mathematics once again indicates the extent of the division between head and hand. The mathematization of science, of which the work of Galileo in the 17th century is the example, established the clear cut division of labour that was previously united in medieval handicraft. Thus Sohn-Rethel links the concept of inertia in Galileo to the economic and social power of capital. This leads to the dissolution of the artisanal mode of production that was lacking the mathematics to solve the new problems. 'Capital and mathematics correlate: the one wields its influence in the fields of economy, the other rules the intellectual powers of social production (p.112).'

The new mode of thinking characteristic of capitalism in the epoch of manufacture emerged with dynamics and the modern science of Galileo. Sohn-Rethel's account of Galileo is largely based on the work of Alexandre Koyré, emphasising the non-empirical character of Galileo's new dynamic principle. This is crucial to Sohn-Rethel's thesis since, in order to illustrate the principle of inertial motion as deriving from the motion contained in the real abstraction of commodity exchange, he juxtaposes a description of the commodity in the market with a description of inertial motion — 'abstract linear movement through abstract, empty, continuous and homogenous space and time, of abstract substances which suffer no material change' (p.128). Inertia remains a static concept until the start of capitalist production when exchange is no longer

confined to the sphere of circulation but brings producers together on the market. When production and exchange can no longer be separated, motion becomes the mode of existence of matter.

In a similar way to that of Harry Braverman, Sohn-Rethel's analysis provides him with the grounds for distinguishing a third stage of the capitalist mode of production. Monopoly capitalism, characterized by major structural changes in the labour process, occurring in pursuit of the intensified valorization of capital, has its own particular intellectual form, exemplified by F. W. Taylor's scientific management. 'The intellectual tasks vested in this management are not seen as representing the workers' minds but as deriving directly or indirectly from science and scientific technology (p. 158).' The ideological assumption that questions cannot be asked about science or its origins places the intellect in a new unassailable position. This new science extends capital's power by representing it as technical expertise. It is not entirely clear whether Sohn-Rethel places Taylor in a similar position to that of Galileo. In other words, should we consider Taylorism as the scientific intellectual form corresponding to monopoly capitalism, or is this intellectual form still basically the same as Galilean science?

It is on the basis of a study of Taylorism that Sohn-Rethel inaugurates the thesis of 'dual economics'. Indeed, the principal new feature introduced by Taylorism under monopoly capitalism is 'the commensuration of labour in action' (p. 170). This stands in opposition to the commensuration of dead labour, characteristic of market economy capitalism and is made into a reality by the process of flow production. It is the conveyor belt that gives reality to social labour commensuration. Taylor's calculations on paper are extended and become reality, for example, in Ford's model of flow production expanded to include agriculture, transport, clerical work, etc. Sohn-Rethel terms the coexistence of these two economies, the one located in the market and 'going back to roots as old as commodity production itself' (p. 163), and the other specific to the most recent form of commodity production, 'the dual economics of monopoly capitalism' (p. 163).

In spite of Marx's not having lived to analyze this third period of capitalism, for Sohn-Rethel it is precisely this period which contains the possibilities of a classless society because of the features contained in the dual economics thesis. Indeed, the social form based on the labour relationship in the production process and deriving its order from that labour process gives such a society a structure that Sohn-Rethel labels 'societies of production'. This bears close resemblance to Braverman's thesis of a science structured by capital, or the incorporation of science into capital. Thus, to Sohn-Rethel, the associated 'logic of production'

(based in monopoly capitalism on the commensuration of living labour) holds the potential of classlessness against the 'logic of appropriation' (based on the commensuration of dead labour), consisting in the exploitation of one class by another. This is the crossroads at which we can choose the socialist road or continue along the capitalist one.

Unfortunately Sohn-Rethel does not elaborate the specific changes in mathematics and science (beyond mentioning statistics and quantum mechanics) that are associated with this expansion of markets and the increase in the rate of exploitation, but prefers to concentrate on the continuities with the original abstraction he defined. However, if juxtaposed with some of the ideas that Luke Hodgkin was developing in *RSJ4*, for example, I feel Sohn-Rethel's thesis would be strengthened. Indeed, Luke Hodgkin's 'new maths' is a vivid example of the logic of production in the sphere of scientific concepts, of the changes in the relations of production of scientific knowledge in their link to the labour process. 'The knowledge/control polarity seems to be absent. Precisely, the mathematics is centred around the achievement of *finite programmes* of work (Hodgkin, p. 57).' It implies the birth of a mathematics very much related to Taylorism.

It is around this type of question that Sohn-Rethel's contribution is most important. Indeed, in spite of this book being necessarily an overview (because it attempts to cover so much, both in time and in scope), I am convinced that the model it offers is one of great value to Marxists. First, the very nature of the project prevents it from excluding any field of intellectual thought from its analysis. It thus attempts to eliminate the problems inherent in a similar undertaking from the position of the 'theory of knowledge'.<sup>3</sup> Sohn-Rethel's Marxism is a more solid basis on which to build a critique of science than is a belief in scientific sociology since it does not itself depend for its validity on a concept of science embodying capitalist ideology and values. Thus it offers the possibility, for example, of a historical materialist analysis of mathematics and science. Even the most 'pure', 'logical' and 'timeless' knowledge can be analyzed historically.

Second, and relatedly, Sohn-Rethel opens the door to the possibility of what could constitute the role of Marxist intellectuals (or such publications as *RSJ?*). Indeed, as long as some areas of knowledge remain excluded from political analysis, there will inevitably continue to exist a debilitating tension between knowledge and production, or between theorizing experience and the experience itself, even among those who are theoretically concerned to abolish that distinction in their own struggles. Sohn-Rethel's achievement is that his analysis of intellectual and manual labour makes it no longer possible to ignore the production of

one's own theories and their origins (although Sohn-Rethel seems to imply this approach rather than carry it out himself). The history of scientific conceptualization becomes indissolubly wedded to the everyday struggles of scientists and workers in general. Since the 'pure intellect' is rooted in commodity exchange, then so are scientific theories and so, indeed, is historical materialism. What makes the latter a weapon of class struggle is not its ability to come closer to any absolute given truth or a superior claim to scientificity, but the social critique implied by its method. This distinguishes it from a pure intellectual form which denies its origin in social being. The material roots of any theorizing must therefore be made explicit. As Gramsci put it, 'the masses "feel" but do not always comprehend or know; the intellectual "knows" but does not always comprehend or feel'. Perry Anderson has noted the increasing distance that has grown this century between Marxism as an abstract ideology and the concrete reality it claims to grasp.

This is certainly one reason why it is important that Sohn-Rethel should have subtitled his book *A Critique of Epistemology*. Kant's epistemology is the kind of idealism that allows the mind and theories a power of their own, independent of their social context. However, it also raises certain problems which are most clearly examined by looking at Sohn-Rethel in parallel to another critic of Kant's philosophical epistemology, the French sociologist Emile Durkheim. In 1938, Celestin Bouglé, a former student of Durkheim's and director of the Ecole Normale Supérieure's Centre de Documentation since 1920, wrote: 'Kantianism was as if reawakened in France after 1870.' Durkheim, though influenced by this reawakening through his connection to the Ecole Normale, was dissatisfied by Kantian *a priori*ism. It attributed the mind's power to organize data to this faculty's being inherent in the human intellect. This did not constitute an explanation for him.

Like Sohn-Rethel, Durkheim needed to relate the genesis and functioning of logical operations to social conditions. Thus he created what he called 'collective representations' and elaborated a sociological epistemology. Kant wrote: 'There can be no doubt that all our knowledge begins with experience . . . . But though all our knowledge begins with experience, it does not follow that it all arises out of experience' (Kemp Smith, p. 41). This is what Durkheim was challenging. He was not only investigating the relationship between forms of thought and forms of society, but was seeking to account for the fundamental conditions to which all thought is subject. Thus, his answer to Kant was that, thanks to sociology, to conserve people's 'distinctive traits, it is no longer necessary to put them outside experience' (Durkheim, p. 447). I would argue that Durkheim went too far.

In attempting to challenge Kant, Durkheim reached towards the opposite extreme and reified the 'social'. Sohn-Rethel falls into a similar trap. What makes this parallel particularly interesting is that the criticisms of Durkheim's sociology concern debates of immediate importance to science, and uncover some of the limitations of Sohn-Rethel's work. It is worth saying something here about Durkheim's relation to positivism. Positivism has been criticized not only for misinterpreting the natural sciences, but also because the whole idea of scientific method presupposes the existence of objects to which this method can be applied. In other words it presupposes that the objects are constituted prior to the knowledge and the application of the method. Durkheim differs from positivists in that he does not operate with a model of universal method but argues for the specificity of the object of his sociology. Nevertheless, like positivism, the object of Durkheim's sociology is a 'given' object. This model of scientific method does not allow for science to constitute its own objects even though this may be in circumstances not of its own choosing.

This point is also made in Gaston Bachelard's idealistic critique of the given-ness of scientific objects. His concept of 'epistemological break' was a reminder that a science was only installed by cutting itself off from its own past, 'that the object of a science is therefore not an immediate given and does not pre-exist the process of its production' (Lecourt, p. 7). In other words, science has no object outside its own activity.<sup>4</sup>

In a similar way to Durkheim, Sohn-Rethel, in opposing the timeless universal appearance of thought form, seems to need to relate it to a social synthesis which itself has to be reified to fill the role of the fundamental condition to which thought is subject. For Durkheim (and Bloor), the only way to give primacy to the 'social' is to give primacy to scientific sociology. The 'social synthesis' does for Sohn-Rethel what 'collective representations' do for Durkheim, that is provide an alternative to *a priori* intuition in Kant.

This argument is important and relates to some of the observations made of Sohn-Rethel's work in *RSJ*2/3. Some of those ought to be mentioned to judge if the greater elaboration that a book allows over an article has enabled Sohn-Rethel to keep his promises. One of these concerns the possibility of a causal account of the parallelisms between commodity formations and scientific conceptions. In other words, Sohn-Rethel promised to make more than 'important analogies' between these. Indeed, he maintained that abstraction, the basis of scientific conceptions, is not limited to the realm of thought but actually *derives* (Sohn-Rethel, 1975, p. 75) from the social process through real abstraction, operated by a human social action, commodity exchange. He

accomplishes this daunting task successfully, for example when he describes the first introduction of coinage around 680 B.C. in Greece as the 'historical origin of conceptual thought in its fully developed form constituting the "pure intellect" in its separation from all men's physical capacities' (p. 67).

In attempting this, however, he puts himself in the position of having to deny charges of succumbing to the opposite extreme of the idealism he is fighting: reflection theory, in which there is a one to one correspondence between the socio-economic base and the intellectual superstructure. But this is also accomplished successfully both in the body of the text and in the final methodological chapter, a vitriolic attack on the theory of reflection where, in spite of acknowledging its political uses, he states: 'I consider its theoretical value to be nil (p. 189).' However, the only way Sohn-Rethel seems able to counter the dangers of reflection theory (i.e. that it treats pure intellect as part of human nature and 'simulates the neutrality of science and technology towards social class' (p. 191)) is by replacing that theory's 'nature' with Marx's 'social being' or his own 'social synthesis' which, although a broader concept, is nonetheless separated as an object from the pure intellect or consciousness his theory is supposed to relate it to.

The problem is Sohn-Rethel's persistence in keeping cognition separate from commodity exchange. This separation is implicit in his conception of what his task is: to complement Marx's critique of political economy with his own critique of philosophical epistemology. That the latter can be seen as lacking, means that Sohn-Rethel has a particular view of Marx's work. This is, that although it deals with how the form and magnitude of value combine to become abstract human labour, it does little more than state the existence of the commodity abstraction and does not explore the source from which it springs. He claims that a critique of Kant can deal with the missing part, the form of value. I would claim on the contrary that a critique of Kant, by recognizing a missing part, necessarily falls prey to the danger of positing an object independent of a method. A more useful task would be to show that Adam Smith's political economy (critiqued by Marx) and Immanuel Kant's philosophical epistemology are *not* 'completely unconnected fields' as Sohn-Rethel states (p. 35) and that the formal aspect of value *cannot* be analyzed 'in separation from its economic content of labour' (p. 9). In spite of putting Hegel on his feet, Marx was influenced by his critique of Kant and refused to make epistemological reflections before the investigation of the concrete content of knowledge.

It may be that the very posing of the problem as one of the relation between two separate objects, base and superstructure, contains this

limitation.<sup>5</sup> It certainly is a very similar criticism to the one made of Durkheim's position. Of course there are indications that this problem preoccupies Sohn-Rethel: 'Additional argumentation will attempt to show that not only analogy but true identity exists between the formal elements of the social synthesis and the formal constituents of cognition (p. 7).' This would have its own problems as mentioned above. But others indicate the justification of my criticism: 'The qualification of that existential reality as "social" derives from the fact that no individual ever commands the conditions of his own existence (p. 200).'

The individual indeed does not feature in Sohn-Rethel's story. His social synthesis is impersonal, and although he can claim that his work 'critically liquidates all the credence on which the ruling classes must rely for the maintenance of their rule' (p. 204), it does so only abstractly. It fails to grasp that credence exercises power not only through its rational appearance but also through the ways in which it is lived by individuals. In other words, the extent to which individuals personally accept or reject the totality of that conditioning. One of the revolutionary aspects of relating, in George Thomson's words, philosophy to money, surely relies also on including the very real human dimension of these living, working individuals. This is especially so given Sohn-Rethel's focus on labour throughout the book. This focus on labour does not seem to include the emotions, the sexual drives, the particularities of labouring individuals. This omission is made all the more noticeable by the potential of the project to link areas of thought and life that are so often held to be discrete and unrelated, and this can only, in my view, diminish the effectiveness of the work.

It nevertheless remains that Sohn-Rethel's achievement is a considerable one in the possibilities it offers. These lie precisely in drawing together previously unconnected fields of investigation in a new way. It is an attempt to theorize the relation of existence to consciousness, overstepping boundaries of specialization and therefore necessarily glossing over certain issues that have often been dealt with elsewhere. However, it is a work about the connections between areas rather than about those areas themselves. My only reservation therefore is that it does not go far enough in this exciting new direction rather than that it is inadequate on certain specific points. Thus, the potential offered by Sohn-Rethel's discussion of Taylorism is not fully realized and developed into what could be termed a 'philosophy of internal relations'<sup>6</sup>, because of the missing dimension described above. This is the only serious criticism that can be made of Sohn-Rethel and one which it is our responsibility to remedy.

## Notes

1. See the excellent history of the Frankfurt School by Jay (1974).
2. On whose work Sohn-Rethel relies heavily.
3. In spite of the considerable achievement of his work, David Bloor has been justly criticized for this. See the excellent review by McNeil (1978) and the longer article by Millstone (1978).
4. It is interesting to note on this issue that an important debate among students of Kant's theory of knowledge has centred around the question of whether Kant believed in a world of 'things in themselves' independent of subjective perception. See, for example, Bird (1962) who disagrees with Prichard (1909).
5. Williams (1977) offers a clear approach to the question of base and superstructure:

'The force of Marx's original criticism had been mainly directed against the *separation* of "areas" of thought and activity (as in the separation of consciousness from material production) and against the related evacuation of specific content — by the imposition of abstract categories. The common abstraction of "the base" and "the superstructure" is thus a radical persistence of the modes of thought which he attacked (p. 78).'

6. This perspective, which has some points in common with Raymond Williams, is developed by Ollman (1971).

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## THINKERS AND DOERS: INTELLECTUALS AND THE POLITICS OF WORK

**Philip Kraft**, *Programmers and Managers: The Routinisation of Computer Programming in the United States*, New York; SpringerVerlag, 1977. 120pp., DM16, ISBN 0-387-90248-1.

**Reviewed by Mike Hales**

The vital and stimulating focus in the writings I'm reviewing here is 'the labour-process'. Over recent years there has been an increasing sense of how important it is to understand what can be meant by socialist practice at the level of everyday workplace activity. In both Britain and the US there seem to be definite movements for 'labour process' politics, which stand in an as-yet unclarified relationship with, say, politics of the State, the Labour Movement, the capitalist-imperialist world market — as propounded in various theories of party organisation; or politics of 'the personal', derived particularly from the womens' movement. It is from within this labour-process tendency that I am writing, and it is from a practical commitment to labour-process politics that my criticisms arise. These criticisms may offend some of you, academics, who read this. So I've tried to explain more of the basis of my criticism in an open letter, also in this issue of *RSJ*. Upset or not, you are invited to take up the issues raised in this review, and so help to strengthen the practical understanding of 'radical science'.

Philip Kraft's book is a good, state-of-the-art 'labour process' approach to the phenomena of computer work. Robert Boguslaw, in the book's Foreword, sums up its value: it is easy to read, but insightful; it is true to

the experience of computer (and other whitecollar) workers in bureaucratic organisations, but talks explicitly of things that are often unrecognised by inhabitants of that world; it helps to demystify the technique-bound world of computing, while contributing to sociologists' debates on bureaucracy and class. On these counts the book is clearly worth reading. As soon as I had read it I passed it on to some analysts and operations staff in the computing department where until recently I worked, confident that they could get some new understanding out of it.

It may seem carping, then, to criticise. But is demystification and partisanship enough? As the author says: the book 'must be considered as a preface to more and better conceived studies of technical occupations and technical workers . . .'. And that is where my feelings of unease begin. Do we, as socialists involved in the radical science movement or scientific workers on the Left, need more (even 'better conceived') *studies* of work? The book seems to illustrate a dangerously seductive mode of intellectual work, which I shall call *detached partisanship*. As an ideological tendency this is very valuable and it represents, as I have said, the current mode of most intellectuals' involvement with the politics of work. But the politics of the approach itself need careful examination. So, yes, I'm going to carp.

However, before I do too much of that I should try to tell you what the book says.

### **What Do Programmers Bosses Do?**

The book's strongest point, and this cannot be emphasised too much, is its detail. It is acutely observed and convincingly conveys the feel of work in contemporary bureaucratic enterprises. It does this in such a way that engineering, scientific and technical, clerical and administrative, and managerial and professional staff (the broad job categories in my old firm) will all find it possible to identify in many ways with the account. For example, the author notes that there is a gender division which operates between the nonprogramming 'programmers' and the programming 'non-programmers' whose status somehow conveniently defies the rational categories of job-classification schemes; and he raises the question of whether it is skills or *salary levels* that become obsolescent with age, in a way that will be all too near the bone for many of the people I used to work with. The recognisability of the conditions of education and work in an engineering-type 'profession' as evoked by the account made me think again, and to some extent afresh, about my own history as a graduate engineer and employee.

I think that Chapter 2, which deals with the institutional production of programming labour-power, would have special force if technical workers were to read it and take it seriously as relevant to themselves. Although Kraft doesn't speak of it in this way, what his account shows is the way in which specific practices in the production of labour-power produce and reproduce the social and hierarchical division of labour. Followed as it is by a more conventional political-sociologist's 'class' analysis of education (in terms of relative access and success of various socio-economic groups), it shows the value of a materialist approach to the politics of professional work. The 'access and success' approach finds itself laying heavy emphasis on the reproduction of class through attitudes, styles of life, and other ideological and cultural systems. But the detailed analysis of material intersections between 'professional' practices in education (which includes, by definition, ideological and cultural intersections) gives a much fuller — and more *actionable* — account.

The detail in the book is such that I find it difficult even now to recall enough about my work a couple of years ago, in order to translate the book's account into the space of my own life. I'm unwilling to accept that there is much legitimate use for this kind of detail as *abstract* knowledge. So it seems obvious to me that such finely-textured accounts of work are wasted if they are not directed at the practices about which they speak, with the conscious aim of giving them a reflexive, theoretical impetus which they otherwise lack. This may or may not have been the case with *Programmers and Managers*. The problem is essentially: how do you make sure that workers *do* learn what intellectuals are capable of learning about them? I'll return to the question at a more general level later.

The force-of-experience strength harbours (very dialectically!) the book's main weakness. Because of its commitment to tell it like it is, to focus and consolidate programming workers' knowledge of programming work, the book constitutes itself as super ideology rather than theory. Let me stress, this is a criticism but not a put-down: one of the most important tasks for intellectuals working with others is 'journalism', in the sense of reflecting-back workers' own awarenesses of their work in such a way that the totality of their experience becomes visible and speakable. Whether or not they can break through into a new and more penetrating description of work, intellectuals could usefully accept a role as organisers of *present* understanding. But more *is* needed; beyond this role as ideologists, we have to aspire to be theorists. Collective self-awareness has to be ordered in such a way that not only the present but also possible futures can be spoken, and there has to be a systematic and integral way of assessing the value of these futures as steps towards a self-conscious revolutionary politics at the level of the workplace. *Programmers and Managers* doesn't offer this kind of advance, and I have no hopes that the ICI programmers

who I know have read the book will be prompted by what they read to organise themselves as a force against ICI-capital. What would be needed for this to be possible is not exhortation or critique, nor even outrage, but rigorous and well-founded ideas of how it might actually be possible to re-order day-to-day practice in ways which are at once more satisfyingly creative, and tend to *un*-reproduce the relations of capitalist production.

In Chapter 1, the author gives a carefully non-mystifying description of what computers do, and poses very strongly the 'paradox' which 'simultaneously divides and unites everyone connected with computers', namely: 'the need for complex preparation in order to make the machines simple to use'. Kraft shows himself sensitive to the complexities of this relation between 'skilled' and 'unskilled' labour, and rejects the run-of-the-mill (managerial) distinction between two types of workers in programming — clericals and managers. He replaces this with a three-way distinction based very much in the daily experience of programmers. The resulting careful and perceptive description is a useful example of a way of analysing the material articulation of forces of production — in scientific work and in other sectors too — for purposes of political organising. But it is not an analysis of the class content of programming practice. Carchedi's *Economic Identification of Classes* is pitched at too high a level of abstraction for political organisation at the workplace, but it indicates a direction in which theoretical work might move. If we did this, we would find that *Programmers and Managers* gave us data for that analysis, rather than substantive parts of the analysis itself.

The complexity of the division of labour, especially the mutual isolation of conceptual and 'non-conceptual' work, is shown in the book. What is not shown is the contradictory class content of the various distinct practices that emerge within programming work. Chapter 4 outlines the structuring of workplace activity into distinct practices, each with its own time-economy, ideology, material product, physical space, culture, and so on. However, what I lacked in my own attempted socialist practice as a programmer, and what Philip Kraft's book lacks, is a way of turning this picture of the forces of production (as a working system of people and artefacts) into a map of the relations of production, as materialised at the immediate level of the labour-process. To act consistently as socialists, workers need a clear grasp of the relative dominance and subordination of different (capitalist and less historically specific) relations of production in their work, of the ways in which these are materially entrenched, and of the extent to which the forces of production in a particular labour process are politically under-determined — that is, conformable with distinct systems of relations of production.

From the point of view of theory, Chapter 3 — 'Deskilling and Fragmentation' — is the central focus of the book. Let me take an example from there, to show what I mean when I say that a more developed concept of the politics of the labour process is wanted. In Chapter 3, three ways are shown by which management seeks to cheapen programming labour-power and to subordinate programming workers. (The two are not identical, which is a point I shall come back to.) Managers of programmers have made extensive use of 'canned programs', that is, pre-produced products of conceptual labour, thus turning a significant proportion of programming work into reproduction rather than production — the reproduction of predetermined structures of concept and practice. Managers have implemented forms of fragmentation and hierarchically divided labour in the collective labour process of programming (in the form of the 'chief programmer team', for example). And they have standardised the conceptual instruments of labour by introducing 'structured programming', which rests upon a strictly limited vocabulary and grammar. Obviously, and Kraft drives the point home, these are analogues of classic strategies adopted in other, 'manual', labour processes to secure the subsumption and subordination of labour under capital.<sup>1</sup>

In the case of structured programming, my own experience suggests that Kraft has been too eager to close the political case, and as a result both theory and potential practice tend to suffer. Programming using a 'structured' subset of the available language can be as great a challenge and as satisfying to the programmer as 'unstructured' programming. I worked for a couple of years with a commercial package for writing simulation programs. The package sacrificed technical efficiency in computation, cost in run-time, and elegance, relative to other available packages; it was quite highly 'structured'. In return it made possible an increased level of intelligibility — with respect to programmers who have to work on the program at a later date, programmers who are not narrow specialists in this language (or in any programming language), and non-programming users with whom the program writer sees the need to communicate. There is a quite real aspect of *openness* about program-writing which can be introduced with such a package; it enhances the ability of a programmer to combine narrow technical work with self-conscious attention to the needs of other *people* who may be implicated in the production and consumption of the product. You will not, I hope, deny that these are values which socialists should be interested in advancing.

OK, I know that this package was bought by the Departmental management because they wished to cheapen simulation work while meeting an increasing demand. Subjectively, the incorporation of that particular instrument of labour into the forces of production in that particular labour-process was determined by the managerial ideology (of centralised

hierarchical control coupled with cost-cutting) which Kraft documents so forcefully. But objectively there was a real ambivalence in the politics of using the package, at the level of my own (relatively unsupervised) work-practice. You will have to take my word for it, but there was scope for manoeuvre within the relations of capitalist knowledge-production. The 'structured' package, as dead labour objectified in software form, did not have to appear as the dominant element in the labour processes of the living labourers who used it. Different relations of conceptual production can be materialised in the 'same' labour power and means of production, thus constituting a *different* system of forces of production which intersects with (and over limited spans may even be identical with) capital's system. The political possibilities at this level are (of course) limited. But they are real, and it is vital that they are investigated, theoretically and in programmers' practice.

However, that whole problematic — of searching for a theoretical understanding which can inform positive subversion as well as outraged resistance — is suppressed from Kraft's book. For example, the alternative possibilities of structured programming are firmly put down in passing references to the Utopian speculations of some software scientists. In view of what I have to say later about the general politics of 'radical' theorists of work, let me say this: what I am asking for is not voluntarism in relation to the objective conditions of wage-labour. I want a serious and theoretically-informed search for a positive, creative strategy for labour which goes beyond protectionism. The forces of production are not, in general, wholly determined by the relations of capitalist production; there *is* class struggle. Certainly, the forces of 'scientific' production are not internally structured in a complete and rigid way; not even by the immediate aspects of wage-labour and private property, let alone by the more abstract relations of value and surplus-value. It is in the under-determined spaces, where practices' limited autonomy operates, that revolutionaries have to make their opportunities, and 'alternative' technologists and 'radical' scientists have to find their politics.

Radicals' discussions of the politics of work are developing in such a way at present that I think it might be productive to impose an embargo on 'deskilling' as a term of *theoretical* discourse. Analyses of deskilling à la Braverman (see References) have had tremendous impact as consciousness-raising, and this same quality is apparent in *Programmers and Managers*. The book highlights the 'paradoxical' relationship between increasing and decreasing skill in different labour processes, and the self-destructive thrust of labour which deskills (as shown in the development of computer hardware and software). But there are distinctions which cannot be made with the radicals' catch-all of 'deskilling'. In Chapter 3 Kraft says

that deskilling is 'the standardisation of work implicated in producing a standardised product'. In itself this is too vague (being very unspecific historically: it is *profitable* standardised products that carry this necessity). However, this sense is not at all the same as 'transforming work made up of . . . interdependent tasks into a larger number of simpler, routine and unrelated tasks'. This is a second — and historical — usage of the term within the same chapter. How the evolution from 'skilled' to 'unskilled' work has taken place in various long-standing — 'craft' work — practices is a question for historical research to answer. It should be distinguished from a more general, and more directly useful question: how are we to conceptualise the dominant and determining structures in the contemporary subordination of workers' practices under management? Apart from the transformation of skilled into unskilled work, there are other forms of recomposition of the collective labourer which appear — at the level of both individual labour-processes and whole economies. For example, the current crisis in the UK newspaper industry, and the demand by managements for the replacement of conventional by computer typesetting with 'single keystroking', seems to be less an attempt at deskilling in any simple sense and more an attempt at writing off existing well-organised collective labourers wholesale in the hope of replacing them with new, 'green' ones.

We need to be able to speak much more clearly of the transformations that take place in labour processes. This means being able to speak more precisely about what it is that gives productive practices their material and historical specificity. In their material aspects (as components of the forces of production in a social formation) we have to be able to say what the use-value of the practice's product is, and what the technical, cultural, psychological, conceptual, etc. structures are which secure the stable reproduction of that product and that labour process. In their historical aspects (as contributors to the reproduction and un-reproduction of the class status-quo) we have to be able to say what relations of production are active in structuring the forces of production, and how *their* mutual relations are ordered (as between, for example, wage-labour and pre-conceptualisation; or exchange-value and hierarchy; or surplus-value and surplus-labour). My own work on the management sciences convinces me that we are a long way from that kind of precision, and a lot of relatively empirical work remains to be done before it will be worth trying to finger 'immanent laws'\* of capitalist labour processes. Certainly, it must be a serious mistake to reduce complex contradictions and tendencies in the practical relations of capital and labour to the single focus of 'deskilling'. Taylorism is not capital's last word, nor craft conservatism labour's only rallying point. I cannot say *what* the present shape of the class struggle is at

\* The term is taken from the Brighton Labour Process Group's important article (see References).

the level of immediate relations in the labour process; but it is surely more complex, more creative (on both sides), and less depressing (Kraft's last chapter offers no hope of advance) than the reductive 'deskilling' approach can grasp.

The amorphous state of theory in this area at present is shown fairly directly in *Programmers and Managers*. The emphasis switches, without it being the author's clear intention, between heavy stress on control (as Kapitalists' Kontrol) and an economistic account which points to cost-reduction as the determining tendency in the evolution of the programming labour process. What is missing is a well-ordered relationship between various possible and necessary levels of theoretical analysis in relation to the class organisation of production. This is very difficult to provide. It is not an omission that Kraft could have been expected to remedy alone; but it would have been useful to find this as a consciously-present absence. In my own work I have found it necessary to distinguish between what I have been calling subordination and subsumption. Subordination is a term for the bringing of workers' *practices* — living labour, concrete labour — under the determination of managers' *practices*. Subsumption connotes the bringing of abstract labour under capital's 'law of value'.<sup>2</sup> What is novel and valuable in the 'labour process' approach is the search for a way to talk about the present and future of subordination — of class struggle at the level of day-to-day work — as distinct from subsumption. This kind of distinction is unspoken in Kraft's book, with the result that some of the most challenging theoretical questions cannot be posed. It is particularly disappointing to see the way that Chapter 3 opens, with a brief and rather overgeneralised economistic analysis of the history of programming. It is not the brevity of the analysis which matters so much as the way that this level of analysis (subsumption) is interjected. It has the effect of cutting across the level of subordination without relating to it in a systematic way. Yet it is that level of subordination which is the implicit theme of the whole book.

To conclude, as regards the specifics of the book. It is straightforward and convincing in its description of the working world of programmers, and for this reason would make a very valuable item on reading lists for workers' discussion groups — if such things could be made to exist. It points insistently and angrily at the reflexive and contradictory relations of 'skill' in what most people regard as a relatively privileged and secure section of the collective labourer. As contributions to the ideological struggle around technical and scientific labour, these are important and are sufficient to establish the book's worth. As a step towards crossing the theoretical limits of Bravermanism, however, *Programmers and Managers* is not the breakthrough that we are, I hope, looking for. Connected with this theoretical shortfall is another (with more obvious practical implications) that is more

general in Left academics' work. This is where I return to what I called *detached partisanship*.

### What Do Academics Do?

To get a statement of the radicals' position in the study of work, which is implicit in *Programmers and Managers*, we have to go elsewhere. At the time when I read the book I had also just read a thesis by another American, Joan Greenbaum (see References). To judge from mutual acknowledgements she is engaged in a related line of work. In that thesis there is a chapter contrasting radicals' theories of work with managerial theories. Pointing to the opposition between managerial power and workplace control, Joan Greenbaum says that:

'Radical theory examines the roots of this struggle and points to ways that work can be reorganised. It looks at the way people try to enhance and upgrade their jobs rather than deskill them. It explores the co-operative and collective work groups that arise in the midst of authoritarian and bureaucratic structures, and it emphasises that fragmented jobs divide the self and weigh upon the consciousness of the individual.'

This is something like a manifesto for the tendency in 'work' theory that I'm writing about (a manifesto, in the sense that none of us gets very far in pointing concretely to ways forward that are acted upon). Its stresses are very important. They take up an explicit class stance in opposition to conventional academic neutralism's implicit class stance, and they emphasise the creativity and autonomy of informal work cultures in opposition to the rationalisation, routinisation and closedness of managerially-policed structures. This theoretical/ideological movement works alternative concepts into the dominant discourse of researchers on work: hierarchy, deskilling, the separation of conception and execution, degradation. In these terms, the class reality of work becomes more speakable, and the mystifications of both 'detached' bourgeois-academic sociology and committed management science can be shown for the class strategies in knowledge that they are. The radical movement in the theory of work is thus innovative: it modifies the (conceptual) *forces* of production within society, taken 'as a whole'. But the movement doesn't of itself constitute a distinct politics. It is the effect of academics' work on the *relations* of production that is at stake here.

Books like *Programmers and Managers*\* and studies like Joan Greenbaum's *In The Name of Efficiency* say a lot about the work and life of the kinds of workers studied, and could conceivably be a real revelation to

\* I could talk about British books in the same way; examples could be the books by Nichols and Armstrong, Nichols and Beynon, Beynon and Willis, which are included in the References.

them. They say rather less about the relations of production of the studies themselves. What *will* the workers learn about themselves as a result of the studies? How do they learn what they learn: do they have to rely on the tenuous and distancing medium of the academic books market, or the lecture hall, to make this link? What, concretely, is the relation between 'radical' theory and the 'non-theoretical' practices studied? For as long as the relations of detachment dominate intellectual production (i.e., commodity relations, and pre-conceptualisation relations) we may, and almost certainly will, get *more* theoretical work on work; but as revolutionaries and socialists we can't expect *better* — just more 'studies' and a burgeoning academic radicalism. This is significant as an ideological current within the professional intellectual sphere, but marginal as far as the transformation of either work relations (even academics' work relations) or other workers' consciousness are concerned. That is, 'radical' studies of work are marginal as a *politics* of intellectual production.

*Programmers and Managers* is interesting in this respect, because this issue of the politics of production seems to have forced itself upon the author in the course of his academic pursuit of objectivity. This study seems to have come about and reached the conclusion it has, at least in part because of Philip Kraft's personal connections and commitments to some programming workers — who also happened to be parts of an object of formal research. So we would expect that at least some of the book's considerable insight has re-entered the practice out of which (and partly in which) it was produced. This is as it should be. That is only guesswork on my part, however; there is room for doubt about just how self-conscious the academic was. I don't think he intended a joke against 'value-free' social science when he raised these issues under the technical-academic heading of 'Expanding The Data Base'. What is at issue is not finding ways of producing knowledge with a greater degree of 'objectivity', on some unidimensional scale: *it is a question of organising to produce knowledges with clear class relations in practice, and rigour sufficient to meet the most exacting practical demands.*

Kraft discovered that: 'programmers and other computer people were at a distinct disadvantage in their dealings with their managers in the matter of how their day to day existence in the workplace was to be organised. Quite frankly, I have become a partisan on the side of programmers.' That step is a large one for even 'radical' academics to make in their published work. It is a first step on to the largely unmapped terrain of the politics of knowledge, and of knowledge's place in the class struggle at the level of work relations. Once academics become aware that this is the ground on which they work, however, the need then becomes one of creating practical connections (on paper, in speech, in personal

collaboration) which effectively challenge those relations of production that are named (albeit inadequately, as yet) in radicals' discourse. Until that stage is reached, there is no way that academics' work on work can claim to be political (as distinct from ideological or theoretical) radicalism.

The problem of producing and conceptually strengthening a breakthrough in the form of intellectuals' work on and *with* other workers confronts workers in Britain and the US alike. As far as published work shows (which is all I have to go on) the game is pretty underdeveloped in both cases. But necessary developments in the US are likely to be hamstrung by what emerges as a tendency in radicals' work towards idealism. In the remainder of this review I want to relate this tendency to what I have already identified as theoretical shortcomings in 'labour process' studies.

In a relatively obvious form we can see this idealist tendency in Stephen Marglin's influential 'What Do Bosses Do?';\* also in the reception that seems to have been given in the States to André Gorz's writing†, and in the response to Braverman's *Labor and Monopoly Capital* itself. The line goes something like this: on one side we have the Bosses, on the other, workers. The bosses and their managerial and technician lackeys are out to screw the workers by reducing them to quasi-machines and eventually replacing them by machines. The bosses have power; but the workers have Job Control. In this radicals' ideology, forms of voluntarism (attributed to managers) combine with forms of spontaneism (as hoped for from workers). So work relations are *really* power relations (which is true), and the development of work is really degradation; intellectuals should tell it like it *is* and have no truck with bourgeois mystifications concerning 'the quality of working life'; and self-reflection in this mirror held up to them by intellectuals will give workers the anger and self-awareness they need as an oppressed class to become a political force and take over the forces of production, rejecting the historically-redundant Kontrol content.

Well OK, that's a caricature. But that idealist tendency does sanction a kind of pseudo-materialism, a no-messing commitment to Telling It Like It Is; and this overlooks the dialectical — and intensely practical — problem of identifying what it might *become*, and how that might be made to arise out of what is. The forces of production in any contemporary technical-scientific practice which we might want to analyse will be structured, to a more or less developed extent, by capitalist relations of production; and maybe other significant relations too, which are not specifically capitalist. Theoretical and ideological problematics, machines, algorithms, formal and informal social groups, architectural

\* This article has circulated in a number of forms. One version is listed in the References.

† Notably the *Telos* article listed in the References.

relationships, etc.: as structures in the forces of production in specific practices in a capitalist social formation, all of these carry some aspect of capitalist production relations. Organising, and establishing a creative revolutionary politics at the level of work, demands an analysis of how the forces of production are effectively shaped as capital; that is of how use-value-for-capital is extracted from labour-power, as distinct from other use-values which are conformable with it. Certainly, the structure and dynamics of subsumption have to be understood for mass politics and, for example, the development of workers' industrial counter-strategy. But there may not be any masses organised, if political activists' grasp of *subordination* does not have the rigour of theory.



At the level of the politics of work, the conceptual opposition, Bosses/Workers, inevitably becomes a reification. At the most abstract levels of analysis, more concreteness is not needed; and, in less rigorous radical thought, that simple opposition serves to sanction a heavy but useful emphasis on the extent to which capitalist management in fact sacrifices economic efficiency for political control. But the simple distinction does not take us beyond this, to provide the conceptual means for grasping the complexities of class at the level of the monopoly-capitalist labour-process (or any other historical labour-process). The working lives of 'workers' and 'managers' are very contradictory things. It is these contradictions in the technical and subjective determination of class content in work which give the political organiser ground to move on. British writers on work are, perhaps, more sensitive to the ways in which the relations of production are entrenched — in all their contradiction — in everyday life. Nevertheless, experience in and out of industry makes me very aware of the gap between theory and 'non-theoretical' workers. There is an urgent need for a theoretically-articulated understanding of how 'power' is materially built into work and work experience through the technical,

conceptual, psychological, cultural and economic organisation of working practice. An approach of that kind could point to the areas of autonomy in practices, relative to the totality of the forces of production, in such a way as to support as creative and innovative reorganisation of workers' thought and time around work.

When I raised this point earlier, I tried to guard myself against charges of voluntarism. The voluntarism and spontaneism in the 'radical' approach that I'm criticising cuts off all considerations such as these I have just raised. Whatever workers may think, it's a bosses world:

'In *Programmers and Managers* I have described structured programming as many managers and programmers see it; I have suggested that management use structured programming to de-skill and control their programmers. Yet, there is nothing inherent in the principles of structured programming . . . which suggests that its developers are concerned with anything except making the writing of programs a more clear-headed and self-conscious undertaking than it presently is . . . Indeed, Dijkstra and Gries have stressed in their writings that they see in structured programming the possibility of enlarging, rather than diminishing, the skills, knowledge and understanding of the average programmer. I have no doubt whatsoever that they believe and hope this to be the case.'

'Unfortunately, this is not the issue. Whatever the intentions of software scientists, it will be managers, not scientists, who decide the manner in which scientific innovations will be applied to the problems of profit-making and employee control.'

For those confronted with the responsibility of *organising* programmers, this is not good enough. To pass as a radical politics, this kind of approach to the conceptualisation of scientific and technical work must draw implicitly on a mysterious belief in the powers of 'the workers'. It carries an unspoken assumption that workers have the power spontaneously to generate a transcendent solution to the problems of wage-labour, so starkly documented — but not *formulated* — by detached partisan intellectuals. Utopian interpretations of methods, equipment and techniques which will, in practice, be implemented on managements' *are* bound to be 'mere' ideology, as Kraft asserts. But the answer is not to write a book showing that those who believe in the liberating potential of 'science' are dupes. To see the political role of knowledge as consciousness raising, outside the day-to-day practice of workers, is to create a self-serving rationalisation of detached academics' production of 'radical' books. There are other forms of knowledge-production, more effective if less spectacular and career-enhancing: discussion groups, co-operative pamphlet writing, research as part of a campaign, journalism on rank-and-file trade-union newspapers, oral history research/workers' creative writing collectives, etc, etc.

The 'epistemological break' that I have been arguing for in this article is a *practical* break. Intellectuals do have very specific and significant contributions to make to the politics of labour processes. But they can not be

made — that is, strongly, the theory can not be developed — by standing outside other workers' practices and carrying out 'studies' which show that they are subordinated to capital, through managerial practice. Only if theorists' and other workers' practices link, materially, is there a possibility of a theory of practice which entrenches a politics of production, by speaking clearly of the re-organisation that must be possible if there is space for revolutionary politics at this level. Such a theory will see, in its own production, the closing of the gap between knowledge, work and class-consciousness.

Demystification and class partisanship are necessary in academics' work, of course. But workers' struggles, and, I think, theory are now moving beyond the point where 'My God, look what they're doing to the workers now' is an acceptable norm for academics' products. Radicals can not, consistently, produce 'studies', whose politics are politics of passive consumption (mediated by commodity relations and the division of mental labour). When we get an alternative practice of knowledge production going for us, whose relations of production are not those of 'detachment', then we can begin to make some sense of 'radical science'.

## NOTES

1. I ought to point out that the language I am using to describe these managerial strategies is not Kraft's. Conceptual labour, production/reproduction, instruments of labour, subordination/subsumption: these are terms of a marxist analysis of 'mental' work. This analysis is hardly well-developed, as yet, but is the direction in which I suggest radicals should be looking for a more rigorous alternative to the vocabulary of 'deskilling'. My own work in this area over the past three years is written up as a DPhil thesis, listed in the References. I apologise for the frequent use of 'rigour' in this article, without explanation. To really sort this one out would take a major article in itself. I use the term in a way which implies exactitude and great attention to form, but not mere formalism or logical-mathematical reductionism. I think that materialists should conceive of rigour as a quality given to thought by specific historical forms of social organisation. I also think that it is possible to describe, in principle, a specifically socialist rigour; but I can't take up the matter here.

2. 'Subsumption' is the term used in the Penguin translation of Marx's 'Results of the Immediate Process of Production' (see References). 'Subordination' is used throughout the Brighton Group's article, which draws upon this work of Marx. I have taken the latter term to denote what

I see as the distinct focus of attention which appears in such recent marxist theoretical work, leaving 'subsumption' to refer to the more conventional levels of analysis within the marxist economic tradition. I shall be discussing the distinction between these levels further in a paper at this summer's Conference of Socialist Economists: 'Theory and Practice: "The Labour Process" and the Politics of Production'.

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RSJ holds informal seminars on the 2nd Wednesday of each month. If you would like to offer a talk, suggest themes or speakers, or get a seminar list, please contact us.

Edward O. Wilson, *On Human Nature*,  
Cambridge, MA, and London, Harvard University Press, 1978,  
Pp.xii+260, \$12.50/£7.85.

**Reviewed by Bob Young**

What is most striking about *On Human Nature* is not its persuasiveness as a scientific treatise; it makes no claim to providing a systematic review of the literature. Rather, its significance lies in its role as a cultural event. The book has brought its author America's foremost literary award, the Pulitzer Prize, and Wilson is now in great demand on the US prestige lecture circuit. Yet the argument of *On Human Nature* is notable for the absence of the extreme claims for biological reductionism and sociobiologic imperialism which characterised *Sociobiology* and the controversy which it engendered. But Wilson is above all that now; the arguments of *On Human Nature* are decidedly muted. He makes no direct mention of his critics and writes in a rather grand, presidential style. All of these features, together with the sumptuous layout and cover of the book and the dignified adverts and lavish bookstore displays which accompanied its publication, lead one to suspect that this is a clear bid for the gentrification of sociobiology. The blunt, competitive, ambitious hustler who rose from the insect to the baboon has succeeded in joining the country club.

This is not to say that the overall effect of the book is not sociobiological but that the mode of its argument is more subtle and—on the surface—modest and balanced. This is a genteel scientism, but it is scientism for all that. Indeed, the only time that the argument returns to some of the feisty passion of his earlier controversies is when scientific naturalism is juxtaposed with religion and with marxism: 'Marxism and other secular religions offer little more than promises of material welfare and a legislated escape from the consequences of human nature (p.3).' 'Marxism is sociobiology without biology... Although Marxism was formulated as the enemy of ignorance and superstition, to the extent that it has become dogmatic it has faltered in that commitment and is now mortally threatened by the discoveries of human sociobiology (p.191).'

But where he formerly made territorial claims to the whole of anthropology and sociology and suggested that ethics stand down until sociobiology recalled it to a much-reduced domain, he is now prepared to live cooperatively. He modestly described the book as 'a deeper and more courageous examination of human nature that combines the findings of biology with those of the social sciences' (p.195). The chapters move onward and upward: Heredity, Development, Emergence, Aggression, Sex, Altruism, Religion, Hope. But he still insists on laying the foundations: 'By a judicious extension of the methods and ideas of neuro-

biology, ethology, and sociobiology a proper foundation can be laid for the social sciences, and the discontinuity still separating the natural sciences on the one side and the social sciences and humanities on the other might be erased (ibid).'

In the end he perpetuates the fundamental confusion between conservative biologism and liberal choice about human nature which was evident in his earlier book: 'We are forced to choose among the elements of human nature by reference to value systems which these same elements created in an evolutionary age now long vanished . . . [This process of choice] will fashion a biology of ethics, which will make possible the selection of a more deeply understood and enduring code of moral values (p.196).'

He sits on the golden back cover, reflecting in the tall grass, manuscript in his lap, the ocean in the background, having completed his great trilogy: *The Insect Societies*, *Sociobiology: The New Synthesis*, *On Human Nature* — all in less than a decade. The last of these is not mere science but 'a speculative essay about the profound consequences that will follow as social theory at long last meets that part of the natural sciences most relevant to it' (p.x). Can a Nobel Prize — Literature? Biology? Peace? — be long withheld?



Opposite is a portrait of Frank B. Baird, Jr., Professor of Science and Curator of Entomology at the Museum of Comparative Zoology.

# NEWS & NOTES

## A REPLY TO JOHN STEWART: GENETIC AND ENVIRONMENTAL FACTORS

**Michael Joseph**

I read with great interest the letter from John Stewart in *RSJ* 8 in which he describes his results indicating a genetic loading for schizophrenia. The evidence he describes should be added to previous persuasive evidence that there is a genetic component in the aetiology of schizophrenia (e.g. de Erven Bohn). No doubt there will continue to be dissension as to whether the genetic evidence is valid. However I wish to address myself to the true point of John Stewart's letter: that if valid evidence for a genetic component in schizophrenia were adduced, then it would pose difficulties for the radical science movement.

In other fields, the suggestion that a disorder might be related both to environment and to predispositions under genetic control would not be exceptionable. For instance it might be demonstrated that the toxicity of a given level of an industrial pollutant, say, will vary from one individual to another, depending upon the rate at which they can metabolize it, which will be partially under genetic control. The radical response would presumably demand a reduction in the level of the pollutant rather than the use of a drug to increase the rate of metabolism and reduce the toxicity. Nevertheless such a drug would be useful in the treatment of, for example, unusually sensitive individuals.

In view of the evidence for the existence of a genetic component in schizophrenia, any model for the causation of schizophrenia could not *a priori* exclude a genetically determined predisposition. On the other hand, only a certain proportion of individuals with this genetic loading develop schizophrenia. Studies from the social psychiatry unit at the Institute of Psychiatry have clearly demonstrated that life-events play a role in the precipitation of schizophrenic episodes and that the usefulness of anti-psychotic drugs depend on the social environment of the patient (cf. Wing).

The relatively stereotyped nature of schizophrenic symptoms, acknowledging cultural modulations, and the relative constancy of incidence across different cultures suggest both that schizophrenia may have a partly physiological origin and that the nature of schizophrenic breakdown may give us insights into the mechanisms of the brain.

Physiological/biochemical models of schizophrenia should take into account both the psychological nature of schizophrenic symptoms and this link between environmental factors and a predisposing weakness. To declare my interest, I work as a biochemist in a unit principally concerned with the exploration of biochemical factors in schizophrenia, and in the mode of action of antipsychotic drugs. In collaboration with two of my colleagues, I am proposing a model of schizophrenia that does have the above features (cf. Joseph *et. al.*). Any model of this type implies that while there may be scope to improve current drug treatment of schizophrenia, such treatment will not be a complete answer as long as social factors are neglected. Even if it were the case that the causation of schizophrenia were purely physical *or* purely social, disturbances in either sphere will affect the other and thus treatments restricted to one are unlikely to be adequate.

It would be unfortunate if the very real contributions that the anti-psychiatry movement has made were interpreted by radical scientists to imply that the cause of schizophrenia is purely social. On the other hand there is the danger that research in biological psychiatry, and models such as ours, will encourage others to assert that schizophrenia is a purely physical problem.

The radical science movement, while maintaining a critical stand on the evidence, should not be pushed into a position of denying the possibility of awkward research findings. They can and should be discussed in terms of the social forces that led these questions to be asked and the ways in which the results are being used. The preconceptions of its opponents are a danger to the radical science movement because they may lead to contrary preconceptions.

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## A REPLY TO JOHN STEWART: WHY DO HIS FINDINGS LOOK AWKWARD?

Les Levidow

The reason John Stewart's findings look awkward for socialists is that he has presented his genetic model for schizophrenia in a deterministic way. Probably without realizing it, he assumes that schizophrenia is a 'thing' some people 'get', and that his statistical investigation can tell us whether it is genes or environment (or a combination thereof) which determines who gets it. Let's look at how such assumptions are embedded within the terms of reference he uses, so that we can understand better why his findings seem to look awkward for socialists, yet why they need not be so.

The key ambiguity in John Stewart's Notes is that his data look 'as if' there really is a 'gene for schizophrenia'. In other words, the data seem consistent with — or could be partially explained by — a model based upon the entity we call a 'gene'. So far, so good — perhaps.

But what is meant by a 'gene for schizophrenia'? Is that term to mean that there is a natural tendency for a particular gene, when 2 doses are present, to cause schizophrenia? And that its exact frequency (24% schizophrenic, 40% schizoid) and choice of victim are determined by separate 'environmental' causes? In other words, does the cause lie — as Michael Joseph has put it — both in a genetically 'predisposing weakness' and in separate 'social factors'? Or — to put the problem entirely differently — could it mean that, given certain social tensions arising from this particular society, those tensions tend to take the form of schizophrenic 'symptoms' when the 2 doses are present?

Through the two alternative meanings I offer, I am not simplistically suggesting that schizophrenia, as some 'thing' people get, has its 'real cause' in capitalist social relations *rather than* in DNA. I don't want to ask which things cause other things, nor which things we must get rid of in order to rid society of schizophrenia, passively acquired as a mere medical condition. Rather, I want to ask what social relations are mediated by — indeed, expressed through — schizophrenia. Only secondarily do I want to ask whether certain DNA's may make it more likely for those social relations to take the form of schizophrenia.

I want to pose the question that way around because deviant social behaviour gets ideologically medicalized, not simply through heritability claims in particular (JS's point), but also through their broader nature/nurture framework. Such a framework demands the collection of data presuming that genes are causally responsible for part of the individual variation in behaviour — indeed, for the very existence of the

behaviour — while presuming that other factors are responsible for the rest of the variation, e.g., as if there were a 'gene for schizophrenia'. So the very term is ideological by virtue of naturalizing particular social relations, of reifying them as a property of genes.<sup>1</sup>

To make my approach more concrete, it's worth recalling the case studies by Laing (and others) of medically-diagnosed 'schizophrenics'. Each of them was an adolescent daughter defending herself against parental aggression/control in apparently incoherent ways. Yet seen in their family context, the girls' behaviour seemed so entirely comprehensible (to Laing, at least) that he refused to take their behaviours as 'symptoms' of a disease. He even refused to ask what 'causes' schizophrenia, taken for granted as a distinct medical condition which somehow happens to befall certain individuals. On the contrary, he saw schizophrenia as socially constructed, in part by the victim herself.

For our own purposes, we would want to ask, for example, why the family institution in a particular stage of capitalism creates conflicts of a sort which are sometimes expressed, not simply through submission, open revolt or escape, but rather through the ambivalence and confusion of 'schizophrenia'. Now it may well be that genetic differences could conceivably help to explain why certain girls deal with their adolescent contradictions by choosing schizophrenia. But whatever may be the worth of such explanations, they cannot possibly explain why schizophrenia occurs in the first place — i.e., why it is those particular sorts of social relations which seem traceable back to genetic differences.<sup>2</sup>

The sorts of 'truths' we learn about nature depend upon the sorts of questions we pose. Enquiries which take for granted the social relations of schizophrenia may well produce data which seem to support a 'genetic model'. But such data need not 'look awkward for socialists' — that is, provided our project is to overthrow the thingified social relations which invariably get divided up into genetic 'nature' and environmental 'nurture'. By refusing such divisions, we have nothing to lose but our reified genes and environment, and a world of communist social relations to win. And if we still find it difficult to leave behind nature/nurture questions, then we need to ask ourselves exactly what it is that we fear losing. Could it be the sort of epistemological security demanded by the psychiatrist who once said, 'If schizophrenia is a myth, it is a myth with a strong genetic component!'<sup>3</sup>

## Notes

1. A common example of such reification is PKU, a genetic defect which impairs mental ability (among others) if not compensated for pharmacologically. Most likely it would do so in any society. However, because the child's development in this society is monitored through IQ testing, PKU has been referred to as a 'low IQ gene', as if it were in the nature of genes for mental ability to take the form of IQ, i.e., as if there were 'genes for IQ', whose natural tendency to produce IQ were impaired by the PKU gene (See my discussion in *RSJ* 6/7.).

By citing the PKU example here, I do not intend to 'disprove', theoretically, *any* genetic analysis of human behaviour. Rather, my intention is to examine how deviance is socially constructed as a deficiency judged according to particular standards of normality, and so to uncover how a particular form of social relations gets attributed to genes.

In that spirit, if we were to take the further example of 'Mongolism' (Down's syndrome), we would need to examine the racist and meritocratic assumptions underlying its social construction as both nomenclature and practice. My most memorable image of Mongolism was a Mongol child's test answer-sheets being talked about at a family-oriented folk dance camp whose social virtue lay in organizing 'ability' in a directly communal, aesthetic way, so as to exclude no-one on grounds of competence. What would it mean to investigate a 'genetic model' for Mongolism, socially defined in terms of 'low ability'?

2. Marx asked an analogous question about how political economy could explain value through labour time. According to what I. Rubin calls Marx's 'genetic' analysis of value, labour time is the content of value, in the sense that magnitudes of value can be traced back to labour time. But labour as such cannot explain value, can't explain why labour takes the form of value in this particular society. Labour, logically speaking, could just as well be the content of some form other than value.

Only (what Rubin calls) Marx's 'dialectical method' can solve the riddle, by asking what social form of labour must uniquely take the form of value. Marx's answer, 'abstract human labour', does not define some empirically quantifiable labour which arithmetically adds up to the values of particular commodities, as if the quantity of some things determined that of other things. Rather, 'abstract human labour' defines the social form of labour which determines that labours will be equalized through the exchange of their products. Hence the central concept is not concrete labour time as such, but average socially necessary labour time. The peculiar form of 'value' is possible only because the social relations of commodity exchange abstract an equivalence out of qualitatively different commodities.

So, in that vein, even if a genetic analysis — be it of value, schizophrenia or IQ — seems to 'explain' quantitative differences among things — prices, symptoms, or test scores, respectively — that sort of analysis cannot possibly explain the origin of the social relations being equated. Only a dialectical analysis can explain the existence of what is being 'measured'.

3. Kety misses the whole point of the 'anti-psychiatry' critique when he proclaims that he has obtained, from his survey of adopted children, 'some results that bear rather directly on the claims of Szasz, Laing, and Roseham that schizophrenia is a myth, that it has no biological substrate, and that it cannot be diagnosed reliably' (p. 960). Somewhat ironically, Kety himself acknowledges partially the *social constitution* of illness in the form of particular diseases when he observes:

'There were fewer hospital beds devoted to mental illness in the large cities of China than in cities of similar size in the West, but it was not felt that the explanation lay in a reduced incidence of mental illness. Instead, it was suggested that Chinese society is more tolerant of mental illness so that patients can be treated in the community and by periodic visits to an outpatient department (p. 958).'

Yet notice how he represents Chinese society's *different social constitution* of illness as simply a different 'treatment' for the same 'mental illness', for the otherwise similar 'incidence' of that illness.

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## WORK COLLECTIVES: UTOPIAN AND OTHERWISE

### Richard Lewontin

Like many others, I have long been exceedingly uncomfortable working in the hierarchically structured organization of the university, yet have been unwilling to leave the university as a work place. As a result, like others, I have participated from time to time, deeply or superficially, in attempts to organize a work place within the university that would be

democratic and collective. I will describe two of these in which I have been directly involved. The first was a brief, and completely abortive attempt; the second a more successful, ongoing collective, that is still not a real success. The failures of both stem from the same contradiction that cannot be solved in the university, or out of it.

The first try was to organize the scientific work of our laboratory along what used to be called 'participatory democratic' lines. When I moved to Harvard from Chicago in 1972, four graduate students from our group also moved, and a new one joined us in Cambridge. The physical facilities in which we were to work had to be designed in the six months before moving, and new technical researchers were to be hired when we arrived in Cambridge. This seemed the perfect opportunity to organize the group along democratic lines. It was decided that the new physical facilities would be designed collectively and with an accent on open joint work space. It was decided that we needed secretarial help, so we collectively engaged in the hiring process, chose a woman who seemed open to collective work, and then she helped to design the physical facilities, especially her own working space. No technical workers were hired, but an attempt was made to share out tasks of stock-keeping, dish washing, lab maintenance, ordering, etc. At the beginning, before physical facilities were built, we shared equally some temporary quarters. Research directions were discussed collectively and grant applications were written by the group. New prospective graduate students were discussed and agreed upon by the group. Some were rejected by the group although I, myself, favored them.

Within a year, after the group had moved into its new quarters, the organisation of work had reverted to a conventional one characteristic of very liberal, but hierarchic, American laboratories. A technical worker had been hired to wash glassware, as well as a laboratory technician chiefly responsible to me. Decisions about how to spend money were now entirely made by me, or at least approved by me. So were decisions about graduate students and post-doctoral fellows. While the atmosphere remained relaxed, with each person carrying out whatever scientific work he or she pleased, and with the laboratory research technician collaborating with different members of the group on a mutually agreed basis, the enterprise was not collective, but rather 'permissive-parental'.

The causes of failure of this attempt were obvious. First, the members of the group had not come together on a political basis, but on a conventional academic one. There was no political agreement and the 'collectivity' had been decreed by the Professor. The students went along, humouring him, but really resenting what seemed to be a way to get them to do a lot of manual and administrative work that others should have

been doing. I, on the other hand, increasingly failed to do my share of the chores because of the press of other work and, secretly, other priorities. The product of the laboratory was alienated from the technical workers, even though the laboratory technician was made a co-author of publications, a sop that could not have a real value to him. The lack of joint political commitment meant that conflicts over the division of labor were not fought out as such, but avoided by the reconstitution of a hierarchical arrangement. Second, whatever the political relations might be, the entire arrangement was in contradiction to the objective situation of power, namely that the Professor had the real power over money, space, work, etc., and only on whom had created the collective attempt. A change in whom could create its opposite. The objective reality of power involves all such attempts at collective work within hierarchical institutions in indissoluble contradictions.

The second attempt to create a work collective has had a firmer base and has been much more successful. A small group of graduate students and post-doctoral fellows had been working within a collective studying agriculture. For reasons that are not directly relevant here, this group decided to separate itself from the other collective and form an independent work unit. I had been loosely associated with the collective, had an institutional position, had helped to obtain and had legal control over part of the grant funds under which they had been working. Therefore they asked me to help form the new collective, and serve as the person through whom the group could deal with the University, and the source of funds. In addition, since there were difficult struggles between the new group and the collective from which it had split for both personal and political reasons, I was asked to try to help resolve these. The result was the final formation of a new working collective, The World Agricultural Research Project, some months later, which included graduate students, post-doctoral fellows, a research assistant, and me. We are still working together as a collective.

The group is held together by a unified political ideology and purpose (with some disagreements in detailed analysis, of course). Two new persons were brought into the collective, on funds already available, because it was decided that we needed someone who could demystify neo-classical economics for us, and someone to take a part of the project concerned with agriculture in socialist and social democratic countries. In addition to research on the political economy of agriculture and agricultural research, the group offers a course on the Political Economy of Public Health in the School of Public Health and writes articles, book reviews and gives collective presentations on agriculture and health.

The collective meets regularly to make all decisions on work, administrative questions and expenditures. Subgroups are sometimes formed for specific work projects but these are brought back to the collective for a full discussion. The course on Political Economy is offered truly collectively despite the official fiction that I am the Professor in charge, and all budget decisions are collective despite the fact that I am legally the recipient of the grant funds. Three of us share what was originally my office space. All people in the group receive the same salary (from grant funds) except, of course, me, who gets his Professorial salary from the University.

Perhaps most important of all as evidence of the collective nature of the group is the remarkable lack of intellectual or political hegemony of one person. There are many disagreements about our work and about analysis of specific issues. Some of these are resolved, some not, but no person or small set of persons in the group usually prevails or sets the tone. Good relations have generally prevailed in no small part because the group has made concessions to the work habits and personal requirements of its members, providing psychic 'space' in which individuals can move fairly freely.

There has been no serious conflict between the collective structure and the University. It is part of the tradition of elite American universities that individual professors may carry on their professional work in whatever idiosyncratic manner they choose. In the end the University will regard the product of our joint work as *my* success or failure, and however decisions are made, the University regards me as the responsible party. There has been some muttering in the School of Public Health about the collective teaching of our course, but having swallowed the camel of a large and popular course from a Marxist perspective, they will not strain at the gnat of collective teaching.

Despite the positive features of the collective, there remain objective asymmetries which have their effect. From time to time some members of the group have complained about a lack of intellectual direction, clearly meaning that I should provide such direction. I have on occasion bowed to this demand by pushing some of the research in a particular direction when some of us seemed at a particularly aimless stage. As representative of the group in various institutional arrangements, I have sometimes acted without consultation and then justified myself after the fact. While sometimes criticized for this, more often this has been accepted without sufficient struggle and discussion. More serious, while other members of the group are essentially completely committed to the work full time (except for each person's outside political work — union organizing, third world activities, publishing a periodical on political economy) I am only

partly committed and spend a good deal of time on other scientific work which is totally unintegrated into the collective work. With my independent salary, I am economically much better off than the rest and am independent by virtue of my institutional position. My participation is a matter of will, so again the objective reality of differential social power makes my relation to the rest of the collective inherently asymmetrical. My personal relations with the rest of the collective are such that I could not, *under present circumstances*, exercise the objective power that is mine; however, if there were a serious political split, I could and would do so. This latent power of an individual in a collective is in irresolvable contradiction with real collectivity and must always be so.

In particular, no one with an institutionalized position of power within a university can possibly be involved in totally collective work within that institution. All of us who attempt to do collective work in universities realize this contradiction and must live with it. We believe that our positions inside these universities are important because the scientific work and some of the educational work that we are able to do only by virtue of our institutional positions are important in political struggles now going on. Moreover, the very power of our positions allows us to provide a shelter for a great deal of work, especially of third world people, that needs some institutional legitimacy as a base. We should not imagine, moreover, that simply moving outside the university resolves the contradictions. Objective asymmetries of power that arise from differential economic independence of members of a group, or from different assumptions of social power that are the legacies of social class, have their effect outside as well as inside institutions. As discovered by the American communal societies of the nineteenth century, stable collective work can only rest on the base of collective necessity.

## RACE & CLASS

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WINTER 1979  
NUMBER 3

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# DIRECTORY OF COMRADELY PUBLICATIONS

## ACHILLES HEEL

Men's Free Press, 7 St. Mark's Rise,  
London E8.  
No. 2(1979): Patriarchy, Men's Groups,  
The Family, Sexuality, 55p.

## ALTERNATIVE PRESS INDEX

Alternative Press Center, P.O. Box 7209,  
Baltimore, MD 21218, USA.

## ANTIPODE—A Radical Journal of Geography

P.O. Box 225, West Side Station,  
Worcester, MA 01602, USA.  
4 issues for \$9, single copy \$2.50.  
Vol. 9, No. 2: Geography & Imperialism,  
Political Economy of Journey to Work;  
Vol. 9, No. 3: Mode of Production and Third  
World Urbanization.

## ARENA—A Marxist Journal of Criticism and Discussion

Box 36, P.O. Greensborough, Victoria  
3088, Australia.  
No. 51(1978) includes David Biggins, 'The  
Social Character of Time';  
No. 52(1979) includes Alan Roberts,  
'Middletown USA—in the Nuclear  
Shadow'; Doug White, 'Structural  
Unemployment: the Meaning of the  
Microprocessor'; David Marcus,  
'Multinational Alignment: the Trilateral  
Commission'. AUS\$2.

## BERKELEY JOURNAL OF SOCIOLOGY

458A Barrows Hall, Dept. of Sociology,  
Berkeley, CA 94720, USA.

## CAHIERS GALILEE

B.P. Galilee 160, 1348 Louvain-la-Neuve,  
Belgium.  
3 issues for 200 frs p.a., 230 frs foreign.  
Sept. 1978 issue includes article on the  
neutron bomb.

## CAHIERS GERSULP

Univ. de Strasbourg, 67070 Strasbourg,  
France.

## CAPITAL & CLASS—journal of the Conference of Socialist Economists (CSE)

55 Mount Pleasant, London WC1.  
Membership rates: £4, low income £2.50,  
overseas £8/£4.  
Australia: subs to Box 161, Sydney NSW  
2001.

## CATALYST—A Socialist Journal of the Social Services.

Box 1144 Cathedral Station, New York, NY  
10025.

## CCCS journal (formerly WPCS), also Women Take Issue: Aspects of Women's Subordination, published by Hutchinson/CCCS.

## CHEMCO NEWS—ICI Shop Stewards' Own Newspaper.

c/o JB Grime, 11 Croft Road, Doncaster,  
S. Yorks DN4 9HA.

## CINE-TRACTS—A Journal of Film and Cultural Studies.

Institute of Cinema Studies, 4227  
Esplanade Ave., Montreal H2W, Quebec,  
Canada.  
4 issues for \$8 p.a., \$10 foreign, \$12 inst.

## COMMENT—libertarian newsletter published by Murray Bookchin, P.O. Box 371, Hoboken, NJ 07030, USA. Send stamped SAE.

## COUNTER-INFORMATION SERVICES (CIS)

9 Poland Street, London W1.  
6 issues for £4, £5 overseas. The Nuclear  
Disaster, 85p; The New Technology, 95p.

## CRIME AND SOCIAL JUSTICE—A Journal of Radical Criminology.

P.O. Box 4373, Berkeley, CA 94704, USA.  
2 issues for \$6, \$10 inst.; foreign \$8/\$12.

## CRITIQUE—A Journal of Soviet Studies and Socialist Theory.

9 Poland St., London W1.  
2 issues for £1.20, \$4 overseas.

## CRITIQUE OF ANTHROPOLOGY

P.O. Box 178, London WC1 6BU.  
4 issues for £3, £6 inst.

**DIALECTICS WORKBOOK**—Marxist Philosophy and the Natural Sciences.  
c/o Hy Cohen, 130 St. Edward St.,  
Brooklyn, NY 11201, USA.

**FUNDAMENTA SCIENTIAE**  
4 rue Blaise Pascal, 67070 Strasbourg,  
France.

**The GUARDIAN**—Independent Radical  
Newsweekly.  
33 West 17th St., New York, NY 10011.  
\$1 for 6 weeks, \$17 p.a.

**HEAD & HAND**—magazine of the CSE  
BOOK CLUB  
55 Mount Pleasant, London WC1.  
6 selections from 1979-80 for £10.50.

**HEALTH/PAC** (Policy Advisory  
Committee) BULLETIN  
17 Murray St., New York, NY 10007.

**HISTORY WORKSHOP**—A Journal of  
Socialist Historians.  
P.O. Box 69, Oxford OX2.  
2 issues for £5 p.a., £6 overseas.  
No. 7 (June 1979) includes Lutz Niethammer  
on 'Male Fantasies: Comments on a New  
Study in History and Psychoanalysis'.

**HMO Packets.**  
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London WC1.  
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claimants, £4 overseas, £1.35 single copy.  
No. 5 (Spring 1979) includes Nik Rose on  
'The Psychological Complex: Mental  
Measurement and Social Administration'.

**IN THESE TIMES**—The Independent  
Socialist Newspaper.  
1509 North Milwaukee Avenue, Chicago,  
Illinois 60622, USA.  
50 issues for \$19 p.a., \$35 inst., \$32 abroad.

**INSURGENT SOCIOLOGIST**  
Dept. of Sociology, Univ. of Oregon,  
Eugene, OR 97403, USA.  
4 issues for \$10 p.a., \$6 low income, \$14  
inst.  
Special Issue on Work & Labor (Fall 1978)  
includes Aronowitz on 'Marx, Braverman  
and the Logic of Capital', double issue \$5.

**INTERNATIONAL JOURNAL OF  
HEALTH SERVICES**  
Baywood Publishing Company, 120 Marine  
St., Farmingdale, NY 11735, USA.  
4 issues for \$25 p.a., \$20 students, \$42 inst.

**The LEVELLER**—an independent  
monthly socialist magazine.  
57 Caledonian Road, London N1.  
£6 p.a.

**LIBERATION**  
186 Hampshire St., Cambridge, MA 02139,  
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**MARXIST PERSPECTIVES**  
The Clomar Corp., 420 West End Avenue,  
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4 issues for \$15 p.a., \$25 inst., \$2 extra  
foreign.

**MEDICINE IN SOCIETY**—Quarterly  
Marxist Journal of Health Studies.  
74 Brookdale Road, London E17.  
Spring 1979 issue includes articles on  
Sickness, Absence and Disability, 30p.

**MONTHLY REVIEW**—An Independent  
Socialist Magazine.  
62 West 14th St., New York, NY 10011,  
USA.  
10 issues for \$13 p.a., \$10 students, \$16  
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**MOTHER JONES**—A Magazine For The  
Rest Of Us.  
607 Market Street, San Francisco, CA  
94105 USA.  
10 issues for \$12 p.a.

**NATURKAMPEN**  
c/o Politisk Revy, St. Pederstraede 28B,  
Copenhagen, Denmark.

**NEW GERMAN CRITIQUE**—An Interdisciplinary Journal of German Studies.

German Dept., Box 413, Univ. of Milwaukee, WI 53201, USA.

3 issues for \$8 p.a., \$16 inst.

No.16 (Winter 1979) includes George Bataille, 'The Psychological Structure of Fascism', \$2.50.

**NEW INTERNATIONAL REVIEW**

P.O.Box 163, Rosedale, NY 11422, USA.

**NEWS FROM NEASDEN**—A Catalogue of New Radical Publications.

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**PRAXIS FOUR**—A Journal of Radical Perspectives on the Arts.

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2 issues for \$7; or from Pluto Press, £4.

**PROKLA**

Postfach 100 529, 1000 Berlin 10, FRG.

**PSYCH-AGITATOR**

Dept. of Psychology, SUNY Stonybrook, NY 11794.

No.5 (Spring 1979) includes articles on The New Sex Therapy, The Madness Industry and Control of Workers, Behaviour Modification.

**QUADERNI PIACENTINE**

Via Poggiali 41, Piacenza 29100, Italy.

**RACE & CLASS**—A Journal for Black and Third World Liberation.

Institute of Race Relations, 247 Pentonville Road, London N1.

4 issues for \$10/£4.50 p.a., \$15/£7 inst.

**RACE TODAY**—Voice of the Black Community in Britain.

74 Shakespeare Road, London SE24.

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**RADICAL AMERICA**

P.O. Box B, North Cambridge, MA 02140, USA.

6 issues for \$10 p.a.

May-June 1979 issue includes Andre Gorz, 'Nuclear Energy and the Logic of Tools', \$2.

**RADICAL EDUCATION**

86 Eleanor Road, London E8.

No.13 (Summer 1979) includes articles on Sin Bins and Social Control, 45p.

**RADICAL HISTORY REVIEW**

Mid-Atlantic Radical Historians' Organization (MARHO), John Jay College, 445 West 59th St., New York, NY 10019, USA.

3 issues for \$12.50 p.a., \$9 unemployed, \$20 inst., plus \$4 extra abroad.

No.20 (Spring 1979), Sexuality in History;

No.21 (Fall 1979), The Politics of Space.

**RADICAL PHILOSOPHERS'**

**NEWSJOURNAL**—A bi-annual journal of Marxism and Socialist Theory, with special features for radicals teaching philosophy.

12 Dartmouth St., Somerville, MA 02145.

3 issues for \$4, \$8 inst.

Fall 1978 issue includes Milton Fisk, 'The Human-Nature Argument', and Richard Schmitt, 'Are Professionals and Managers a Separate Class?'

**RADICAL PHILOSOPHY**

40 Langdon Park Road, London N6.

3 issues £2 p.a., £4 inst., overseas

£2.50/£5.50.

No.22 (Summer 1979) includes Les

Levidow, 'Towards a Materialist Theory of Ideology: the IQ Debate as a Case Study'.

**RADICAL STATISTICS**, bulletin of BSSRS.

9 Poland St., London W1.

No.15 includes debate on 'The interpretive attack on statistics' and on fee-for-service, as well as reports from sub-groups on education, health and race.

**RED EYE**

Box 1200, 2000 Center St., Berkeley, CA 94704, USA.  
Libertarian Communist magazine. Articles on the international crisis, resistance to austerity, culture, the left and the ultra-left.

**RED LETTERS—Communist Party**

Literature Journal.  
16 King St., London WC2.

**REVIEW OF AFRICAN POLITICAL ECONOMY**

Onyx Press, 27 Clerkenwell Close, London EC1.  
3 issues for £3, \$9 overseas, £2 Africa.

**REVIEW OF RADICAL POLITICAL ECONOMICS**

URPE, 41 Union Square West, Room 901, New York, NY 10003, USA.

**REVOLUON—Tijdschrift Over Technologie Natuurwetenschappen en Kapitaal.**

Postbus 1328, 6501 Nijmegen, Holland.  
4 issues for fl17.50 p.a.

**REVOLUTIONARY SOCIALISM—Big Flame magazine.**

217 Wavertree Road, Liverpool 7.  
No.3 includes articles on the trade unions, Labour Party and Italy.

**The RIPENING OF TIME**

Revolutionary Struggle, c/o M.McBride, 5 Henrietta St., Dublin 1, Ireland.  
6 issues for £2, single copy 50p.  
Also, 'Nuclear: the Impossible Gamble'(30p) includes article from *RSJ* on Harrisburg & 'safe level'.

**SAPERE**

Galleria Strasburgo 3, 20122 Milano, Italy.  
12 issues for 12,000 It.L.  
December 1978 issue on nuclear power.

**SCIENCE & SOCIETY**

Room 4331 John Jay College, CUNY, 445 West 59th St., New York, NY 10019, USA.  
4 issues for \$10 p.a., \$15 p.a. inst., \$2 extra foreign.  
Summer 1978 issue includes Beatrice Lumpkin, 'History of Mathematics in the Age of Imperialism'.

**SCIENCE BULLETIN—A Quarterly Journal of Science, Society and the Struggle for Socialism.**

27 Bedford St., London WC2.  
£2 for 4 issues, 40p single copy.  
No.22, Science & Ideology issue, includes critique of *RSJ* by Hilary & Steven Rose.

**SCIENCE FOR PEOPLE—Magazine of BSSRS.**

9 Poland St., London W1.  
Issue 42 includes The Other Einstein, Workers' Control of Technology, and Feminist Perspective on Food; Issue 43 contains talks from BSSRS conference on 'Science Under Capitalism', including Werskey, Young and Shallice.

**SCIENCE FOR THE PEOPLE—magazine of SESP.**

897 Main Street, Cambridge, MA 02139, USA.  
6 issues for \$6 p.a., or \$15 membership.

**SCIENCE FOR THE VILLAGES**

c/o Anand Kumar, Magan Sangrahalaya, 442001 Wardha Maharashtra, India.

**SIGNS—Journal of Women in Culture and Society.**

University of Chicago Press, 5801 Ellis Avenue, Chicago, Illinois 60637, USA.  
4 issues for \$15 p.a., single copy \$4.  
Autumn 1978 special issue on Women, Science and Society—includes Donna Haraway on 'Animal Sociology and a Natural Economy of the Body Politic', a critique of physiology/primate behaviour studies.

**SOCIAL ALTERNATIVES—A New International Magazine.**

Dept. of External Studies, Univ. of Queensland 4067, Australia.  
\$8 p.a., \$12 inst., \$2.50 single copy.

**SOCIAL TEXT—Theory/Culture/Ideology**

700 West Badger Road, Suite 101, Madison, WI 53713, USA.  
3 issues for \$8 p.a., \$18 inst.

# **SOCIALISM AND EDUCATION—**

Journal of the Socialist Educational Association.  
198 Eastcombe Avenue, London SE7.  
3 issues for £1.20 p.a.; membership £4.  
Summer 1979 issue includes articles on  
Disruption and Special Units, 55p.

# **SOCIALIST REVIEW**

New Fronts Publishing Co., 4228 Telegraph  
Ave., Oakland, CA 94609, USA.  
6 issues for \$15 p.a., \$30 inst., \$3 single  
copy.  
Forthcoming articles include 'Nuclear  
Power and the Anti-Nuclear Movement',  
'Television and Culture'.

# **SOCIALIST TEACHER—**

Journal of the  
Socialist Teachers Alliance.  
c/o Archie Faulds, 52 Langley Road,  
Harrow, Middx.  
No. 8 (Summer 1979) includes articles on  
race & IQ, Science Education, 45p.

# **SPARK—**

Committee for Social  
Responsibility in Engineering (CSRE).  
475 Riverside Drive, New York, NY 10027.  
\$6 p.a., \$10 inst.

# **SYGHRONA THEMATA**

c/o Kosta Gavroglu, Esperou 32, Paleon  
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# **TEACHERS' ACTION**

2 Turquand St., London SE17  
3 issues for £1.20.  
No. 12 includes articles on Falling Rolls,  
ESN Schools & IQ Testing, Disruptive  
Units.

# **TECHNIKERGRUPPE "KATALYSE"**

Wittenbergstrasse 14, 4300 Essen, FRG.  
**TECHNOLOGY & CULTURE**  
University of Chicago Press, Chicago,  
Illinois.

Jan. 1976 issue includes Ruth Schwartz  
Cowan, 'The "Industrial Revolution" in the  
Home: Household Technology and Social  
Change in the 20th Century'.

# **TELOS—**

A Quarterly Journal of Radical  
Thought.  
Sociology Dept., Washington Univ.,  
St. Louis, Missouri 63130, USA.  
4 issues for \$12 p.a., \$20 inst., US currency  
only.  
No. 38 (\$3.50) includes Mihaly Vajda on  
'Lukács' and Husserl's Critique of Science'.

# **TESTI E CONTESTI—**

Quaderni di  
Scienze, Storia e Società.  
CLUP, piazza Leonardo da Vinci 32,  
Milano, Italy.  
4 issues for lire 11,000.  
No. 1 (May 1979) includes articles on  
Weimar science, scientific management and  
Einstein, lire 3000.  
No. 2 includes articles on the 2nd Industrial  
Revolution, Mach, the 'Progressive Era', as  
well as translations of Bob Young, 'Getting  
Started on Lysenkoism', and Les Levidow,  
'Towards a Materialist Theory of Ideology:  
the IQ Debate as a Case Study'.

# **UNDERCURRENTS**

27a Clerkenwell Close, London EC1.

# **EL VIEJO TOPO**

Caspe 78, 3<sup>o</sup> 2<sup>a</sup>, Barcelona, Spain.  
Single copy 100 ptas.

# **WECHSELWIRKUNG—**

Technik/  
Naturwissenschaft/Gesellschaft.  
c/o Reinhard Behnisch, Hauptstr. 31, 1000  
Berlin 62 FRG.  
4 issues for 20 DM.  
Jan. 1979 on social effects of new  
technology.

# **WETENSCHAP EN SAMENLEVING**

VWO, Stadhouderslaan 91, 3583 JG  
Utrecht, Holland.  
10 issues for f35.

# **WEDGE**

44 Eccles Road, London SW11.  
No. 3 (Winter 1978). Looking at the Left,  
75p. Includes Martin Thom, 'Anti-racism:  
infections of language'.

# **WORKING PAPERS**

(formerly Studies in  
the discourses of sex, subjectivity and  
power)  
Feral Publications, Box 83, Wentworth  
Building, Sydney University 2006,  
Australia.  
Published so far: Michel Foucault: Power,  
Truth, Strategy, \$5.50; Language, Sexuality  
and Subversion, \$4.95; \$8.00 p&p.

## PUBLICATIONS RECEIVED AND NOTICED

(Published in London unless otherwise noted; prices subject to change.)

- William E. Akin, *Technocracy and the American Dream: The Technocracy Movement 1900-1941*, University of California Press, 1977. Pp.xvi+227. £7.25. ISBN 0 520 03110 5.
- Michael Albert, *What Is To Be Undone: A Modern Revolutionary Discussion of Classical Left Ideologies*, Boston, Porter Sargent pb, 1974. Pp.x+336. \$3.95. ISBN 0 87558 076 9.
- Angelo Baracca, Stefano Ruffo and Arturo Russo, *Scienza e Industria 1848-1915: Gli Sviluppo Scientifici Contesi alla Seconda Rivoluzione Industriale*, Roma-Bari, Editori Laterza, 1979. Pp.xiv+347. lire 5000.
- Barry Barnes and Steven Shapin (eds.), *Natural Order: Historical Studies of Scientific Culture*, Sage Publications pb, 1979. Pp.225. £5.00. ISBN 0 8039 0959 4.
- Maxine Berg (ed.), *Technology and Toil in Nineteenth Century Britain: Documents*, CSE Books pb, 1979. £3.50 (cheaper if you join CSE Bookclub—see advert in this RSJ). Pp.viii+246. ISBN 0 906336 03 1.
- 50 accounts of the organization of work in nineteenth-century Britain, bringing together analytic and evocative materials re the dialectic between technology and the labour process.
- Jeremy Bernstein, *Experiencing Science*, Burnett Books, 1979. Pp.275. £5.95.
- Essays on science from the *New Yorker*.
- Ned Block and Gerald Dworkin (eds.), *The IQ Controversy: Critical Readings*, Quartet pb, 1977. Pp.xiii+557. £4.95. ISBN 0 7043 3133 0.
- The best collection of readings on the topic.
- Stephen Boddington, *Science and Social Action*, with an Introduction by Mike Cooley, Allison and Busby, 1978. Pp.190. £6.95.
- Daniel J. Boorstin, *The Republic of Technology: Reflections on Our Future Community*, Harper & Row pb, 1978.
- Walter Bowart, *Operation Mind Control*, Fontana pb, 1978. Pp.317. £1.00. ISBN 0 00 635241 3.
- A popular account of the activities of the CIA in the technology of behaviour control.
- Serge Bricianer, *Pannekoek and the Workers' Councils*, St. Louis, Telos Press pb, 1978. Pp.304. \$4.00. ISBN 0 914386 18 2.
- Presents works of an important figure in the libertarian socialist movement for council communism, particularly relevant for the struggle for self-management at work and throughout society.
- E. Richard Brown, *Rockefeller Medicine Men: Medicine and Capitalism in America*, University of California Press, 1979. Pp.283. \$12.95. ISBN 0 520 3817 7.
- Hilde Bruch, *The Golden Cage: The Enigma of Anorexia Nervosa*, N.Y., Vintage pb, 1979. Pp.xvi+160. \$2.50. ISBN 0 394 72688 X.
- BSSRS Politics of Health Group, *Food and Profit: It Makes You Sick*, BSSRS (9 Poland St., London W1) pamphlet, 1979. Pp.28. 50p+15p postage.
- Arthur L. Caplan (ed.), *The Sociobiology Debate: Readings on the Ethical and Scientific Issues concerning Sociobiology*, with a foreword by E.O. Wilson, Harper Colophon pb, 1978. Pp.xiv+514. \$6.95. ISBN 0 06 090627 8.
- 46 selections and articles, historical, expository, philosophical, political, including main Science for the People critiques.
- Nancy Chodorow, *The Reproduction of Mothering: Psychoanalysis and the Sociology of Gender*, University of California Press, 1978. Pp.viii+264. £9.00. ISBN 0 520 03133 4.
- A feminist approach, based on object relations theory.
- Stephen L. Chorover, *From Genesis to Genocide: The Meaning of Human Nature and the Power of Behavior Control*, MIT Press, 1979. Pp.xiv+238. \$15.00. ISBN 0 262 03068 3.
- Don't be put off by the 'Plato to NATO' title; analyses human nature as a social weapon; IQ as a social control; politics of psychologists in defining, measuring and controlling US melting pot; drugs and behaviour; violence to the brain for pacification; crime and psychotechnology.

- Olivia Clark, Jerry Lembke and Bob Marotto, Jr., *Essays on the Social Relations of Work and Labor*, a Special Issue of *The Insurgent Sociologist* (vol.8, nos. 2&3, Fall, 1978). Pp.216. \$5.00.  
See especially articles on Labour, Technology and Social Relations, including Marx & the Labour Process, Social Relations of Production and Stanley Aronowitz on 'Marx, Braverman and the Logic of Capital' which is gratifyingly convergent with the RSJ work on science, technology and the labour process.
- Benjamin Coriat, *L'Atelier et le Chronomètre: Essai sur le Taylorisme, le Fordisme et la Production de Masse*, Paris, Christian Bourgois Editeur pb, 1979. Pp.299. n.p. ISBN 2 267 00154 3.
- Philip Corrigan, Harvie Ramsay and Derek Sayer, *Socialist Construction and Marxist Theory: Bolshevism and Its Critique*, Macmillan Press, 1978. Pp.xviii+232. £8.95 (but £4.95 from CSE Bookclub). ISBN 0 333 21245 2.  
Not an obscure tract on Stalinism but an important critique of the theory of the productive forces, i.e., the basis for vulgar marxist economism, technicism and scientism; see sensitive review in *Capital & Class* 8 by Mark Harrison, pp.148-52.
- Stephen Croall and Kaianders Sempler, *Nuclear Power for Beginners*, Writers & Readers' Publishing Co-op pb, 1978. Pp.168. £1.80. ISBN 0 906386 00 4.  
Cartoon and text Beginner Book.
- C.D. Darlington, *The Little Universe of Man*, Allen & Unwin, 1978. Pp.307. £6.95. ISBN 0 04 570010 9.  
Handy one-volume reactionary scientism on everything that lived, lives or will live. Malthus lives! (in retirement in Oxford).
- Dorothy Dinnerstein, *The Mermaid and the Minotaur: Sexual Arrangements and Human Malaise*, Harper Colophon pb, 1977. Pp.xvi+288. \$3.95. ISBN 0 06 090587 5. Published in Britain as *The Rocking of the Cradle and the Ruling of the World*, Souvenir Press/Condor Books pb, 1978. £2.95. ISBN 0 285 64854 3.  
An important (if sometimes portentously worded) exploration of the relations between child care arrangements and the existing order of society; highly recommended by RSJ Marx/Freud subgroup.
- Elisabetta Donini, Arcangelo Rossi, Tito Tonietti (eds.), *Matematica e Fisica: Struttura Ideologia*, Bari, Italy, de Donato pb, 1977. Pp.335. lire 7500.
- Lesley Doyal with Imogen Pennell, *The Political Economy of Health*, Pluto Press pb, 1979 (Sept.). £4.95. ISBN 0 86104 086 4.
- Herbert L. Dreyfus, *What Computers Can't Do: The Limits of Artificial Intelligence*, revised ed., Harper Colophon pb, 1979. Pp.xiv+354. \$5.95. ISBN 0 06 090613 8.
- Pierre Dubois, *Sabotage in Industry* (1976), Penguin pb, 1979. Pp.224. £1.25. ISBN 0 14 02 2127 1.
- Barbara Ehrenreich and Deirdre English, *For Her Own Good: 150 Years of the Experts' Advice to Women*, Pluto Press pb, 1979. Pp.x + 325. £3.60. ISBN 0 86104 062 7.
- Dave Elliott with Pat Coyne, Mike George and Roy Lewis, *The Politics of Nuclear Power*, Pluto Press pb, 1978. Pp.142. £1.95. ISBN 0 86104 028 7.
- Jon Elster, *Leibniz et la Formation de l'Esprit Capitaliste*, Paris, Aubier Montaigne pb, 1975. Pp.256. n.p. ISBN 2 7007 0018 X.
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Working Papers series, replacing Working Papers: Studies in the Discourses of Sex, Subjectivity & Power.
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*Signs: Journal of Women in Culture and Society*, Special Issue on 'Women, Science, and Society' (Vol.4, no.1, Autumn 1978), University of Chicago Press. Pp.216. \$4.00. ISSN 0097 9740.

Contains many interesting articles on women and science, especially a two-part study by Donna Haraway on 'Animal Sociology and the Natural Economy of the Body Politic', the importance of which is difficult to overestimate for the study of the ideological constitution of the content of scientific knowledge—in this case in the study of animal behaviour.

D.A. Smart (ed.), *Pannekoek and Gorter's Marxism*, Pluto Press pb, 1978. Pp.176. £2.95. ISBN 0 904383 71 7.

The editor provides a lucid introduction to a set of texts by the main theorists of council communism, defending self-organisation against social democrats, Bolsheviks and anarchists.

Alfred Sohn-Rethel, *Economy and Class Structure of German Fascism*, CSE Books pb, 1978. Pp.159. £2.50. (available cheap through CSE Bookclub). ISBN 0 906336 01 5.

Considers the relationships between economics and fascist ideas and politics from the vantage-point of a marxist who had infiltrated the upper echelons of German big business in the 1930s.

Barbara Stocking and Stuart L. Morrison, *The Image and the Reality: A Case-Study of the Impact of Medical Technology*, Oxford University Press for the Nuffield Provincial Hospitals Trust pb, 1978. Pp.xii+80. £3.00. ISBN 0 19 721220 4.

The development and marketing of the EMI body scanner.

E.P. Thompson, *The Poverty of Theory and Other Essays*, Merlin Press pb, 1978. Pp.v+406. £3.90. ISBN 0 85036 232 6.

If you want to have an understanding of the relationship between old left positions, new left arrogance, libertarian marxism and current theoreticist scientism, this is the volume to read. Thompson's fierce, ruminative and often hilarious essays trace his reactions to Stalinism, the *New Left Review*, keeping the faith, and Althusserianism, opening up the texture of marxist theory and struggle over the past three decades. He is still too empiricist and has not taken science into his humanist marxist vision, but it's still very important work for finding a way among the marxisms.

Carlo Violi, *Galvano Della Volpe: Testi e Studi (1922-1977)*, Messina, Editrice La Libra pb, 1978. Pp.xxx+428. lire 10000.

Pat Walker (ed.), *Between Capital and Labour*, Hassocks, Sussex, Harvester Press pb, 1979. Pp.xxviii+335. £3.95. ISBN 085527 8277. Published in America—Boston, South End Press pb, \$4.75. ISBN 0 89608 037 4.

Texts on the debate about the existence and political potential of what John & Barbara Ehrenreich call 'the Professional-Managerial Class', including experts, e.g., scientific, technical and medical workers, teachers; here criticised by, e.g., David Noble, E.O. Wright, Stanley Aronowitz.

A very important set of issues for *RSJ* readers on the contradictions of our own class positions and the problems thereby raised for our role in class struggle.

David Widgery, *Health in Danger: The Crisis in the National Health Service*, Macmillan Press/Papermac pb, 1979. Pp.xx+178. £2.95. ISBN 0 333 23178 3.

Raymond Williams, *Keywords: A Vocabulary of Culture and Society*, Fontana pb, 1976. Pp.286. 95p. ISBN 0 00 633479 2.

Ruminative definitions of main words in marxist and cultural discourse, including historical development of meanings.

Raymond Williams, *Politics and Letters: Interviews with New Left Review*, New Left Books, 1979. Pp.444. £12.75. ISBN 86091 000 8.

These exploratory and reflective interviews are likely to be as important to the development of marxist thought in the 1980s as the revival of Lukács in the 1970s and the scholarship and reflective writings of E.P. Thompson. The interviews cover his life and times, cultural theory, drama, literature, and politics, and they offer the reader a way into the issues and contradictions which have faced the left in recent times and face us in the future. Williams' work is as significant for the theorisation of science and technology as it is for the rest of culture and society.

Elizabeth R. Zetzel and W.W. Meissner, *Basic Concepts of Psychoanalytic Psychiatry*, N.Y., Basic Books, 1973. Pp.viii+312. n.p. ISBN 465 00571 3.

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## CONTRIBUTORS

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